SOME AMAZING MAZES.

[CONCLUSION.]

EXPLANATION OF CURIOSITY THE FIRST.

You remember that at the end of my description of the card "trick" that made my first curiosity, I half promised to give, some time, an explanation of its rationale. This half promise I proceed to half redeem.

Suppose a prime number, P, of cards to be dealt into S (for *strues*) piles, where $S<P$. (Were $S=P$, it would be impossible to regather the cards, according to the rule given in the description of the "trick.") Then, in each pile, every card that lies directly on another occupied, before the deal, the ordinal, or serial, place in the packet whose number was S more than that of the other; and using Q to denote the integral part of the quotient of the division of P by S, so that $P-QS$ is positive, while $P-(Q+1)S$ is negative (for P being prime, neither can be zero,) and assuming that the piles lie in a horizontal row, and that each card is dealt out upon the pile that is next on the right of the pile on which the last preceding card was dealt, it follows that the left-hand piles, to the number of $P-QS$ of them, contain each $Q+1$ cards, while the $(Q+1)S-P$ piles to the right contain each only Q cards. It is plain, then, that, in each pile, every card above the bottom one is the one that before the dealing stood S places further from the back of the packet than did the card upon which it is placed in dealing. But in what ordinal place in the packet before the dealing did that card stand which after the regathering of the piles comes next in order after the card which just before the regathering of the piles lay at the top of any pile whose ordinal place in the row of piles, counting from the left, may be called the sth? In order to answer this question, we have first to consider that the effect of Standing Rule No. IV is that the pile that comes next after any given pile in the order of the regathered packet, counting, as we always do, from back to face, is the pile which was taken up *next before* that

given pile; and of course it is the bottom card of that pile to which our question refers. Now the rule of regathering is that, after taking up any pile we next take up, either the pile that lies $P-QS$ places to the right of it, or else that which lies $(Q+1)S-P$ places to the left of it. In other words, the pile that is taken up *next before* any pile, numbered s from the left of the row, is either the pile numbered $s+QS-P$ (and so lies toward the *left* of pile s) or else is the pile numbered $s+(Q+1)S-P$ (and so lies toward the *right* of pile s). But if pile number s were one of those which contain $Q+1$ cards each, since these are the first $P-QS$ piles, we should have $s \leq P-QS$, and the pile taken next before it, if it were to the left of it, would be numbered less than or equal to zero; and there is no such pile. Consequently in that case, that pile taken up next before pile s will be to the right of the pile numbered s, and its number will be $s+(Q+1)S-P$, which will also have been the number of its bottom card in the packet before the dealing; while, since the bottom card of pile number s was card number s before the dealing, and since this pile contains Q other cards, each originally having occupied a place S further on than the one next below it in the pile, it follows that its top card was, before the dealing, the card whose ordinal number was $s+QS$. Thus, while every other card of any of the first $P-QS$ piles is followed after the regathering by a card whose original place was numbered S more than its own, the top card of such a pile will then be followed by a card whose original place was S more than its own, *counting round a cycle of* P *cards.* In a similar way, if pile number s contains only Q cards, it is one of the last $(Q+1)S-P$ piles. Then it cannot be that the pile taken up, according to the rule, next before it lay to the right of it; for in that case the number of this previously taken pile would exceed S. It must therefore be pile number $s+QS-P$; and this will be the original number of its bottom card, while the original number of the top card of pile number s (since this contains only Q cards,) will be $s+(Q-1)S$. Hence, as before, the top card will be followed after the regathering by a card whose original place would be S greater than its own, but for the subtraction of P in counting round a cycle of P numbers. This rule then holds for all the cards.

It follows that if, after the regathering, the last card, that at the face of the pack or in the P place is the one whose original place may be called the Πth, then any other card, as that whose place after the gathering is the lth, was originally in the $\Pi+lS-mP$, where mP is the largest multiple of P that is less than $\Pi+lS$. If

however, after the regathering, the pack be cut so as to bring the card which was originally the Pth, or last, that is, which was at the face of the pack, back to that same situation, then, since the original places increase by S (round and round a cycle of P places) every time the regathered places increase by 1, it follows that the original place of the card that is first subsequently to that cutting will have been S, that of the second, 2S, etc.; and in general, that of the lth will have been $lS-mP$. If the cards had originally been arranged in the order of their face values, the face value of the card in the lth place after the cut will be $lS-mP$, which we may briefly express by saying that the dealing into S piles with the subsequent cutting that brings the face card back to its place, "cyclically multiplies the face-value of each card by S," the cycle being P. If after dealing into S piles, another dealing is made into T piles, and another into U piles, etc., after which a cut brings the face card back to its place, the face value of every card will be cyclically multiplied by $S \times T \times U \times$ etc. Moreover, if cuttings were made before each of the dealings, since each cutting only cyclically adds the same number to the place of every card, the cards will still follow after one another according to the same rule; so that the final cutting that restores the face card to its place, annuls the effect of all those previous cuttings.

My hints as to the rationale of the exceptional treatment of the last card in twelve initial deals, and as to the extraordinary relation which results between the orders of succession of the black and of the red cards must be prefaced by some observations on the effects of reiterated dealings into a constant number of piles. What I shall say will apply to a pack of any prime number of cards greater than two; but to convey more definite ideas I shall refer particularly to a suit of 13 cards, each at the outset having its ordinal number in the packet equal to its face-value. The effect of one cyclic multiplication of the face-values by 2, brought about by dealing the suit into 2 piles, regathering, and cutting, if need be, so as to restore the king to the face of the packet, will be to shift all the cards except the king in one circuit. That is, the order before and after the cyclic multiplication being as here shown.

Before the cyclic doubling of
the face values ....1, 2, 3, 4, 5, 6, 7, 8, 9, X, J, Q, K,
After the same ....2, 4, 6, 8, X, Q, 1, 3, 5, 7, 9, J, K,

the 2 takes the place of the 1, the 4 that of the 2, the 8 that of the 4, the 3 that of the 8, the 6 that of the 3, the Q that of the 6, the J that of the Q, the 9 that of the J, the 5 that of the 9, the X that of the

5, the 7 that of the X, and the 1 that of the 7; so that the values are shifted as shown by the arrows on the circumference of the circle of Fig. 6. If 7, instead of 2, be the number of piles into which the thirteen cards are dealt there will be a similar shift round the same circuit, but in the direction opposite to the pointings of the arrows; and if the cards are dealt into 6 or into 11 piles, there will be a shift in a similar single circuit along the sides of the inscribed stellated polygon. But if the 13 cards are dealt into a number of piles other than 2, 6, 7, or 11, the single circuit will break into 2, 3, 4, or 6 separate circuits of shifting. Thus, if the dealing be into 4 or into 10 piles, there will be two such circuits, each along the sides of a hexagon whose vertices are at alternate points along

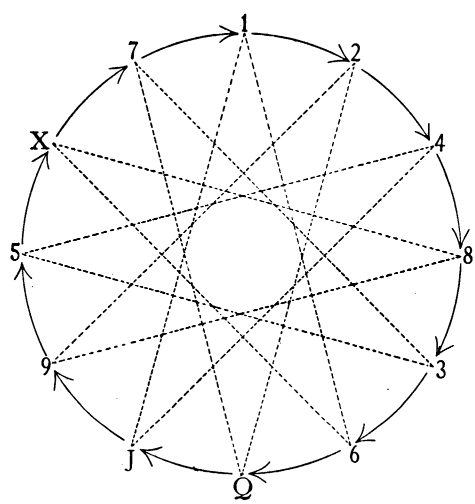

Fig. 6.

the circumference of the circle in the same figure (or, what comes to the same thing, at alternate vertices, along the periphery of the stellated polygon). Dealing into 4 piles makes one round from 1 to 4, from 4 to 3, from 3 to Q, from Q, to 9, from 9 to X, and from X back to 1; while another round is from 2 to 8, from 8 to 6, from 6 to J, from J to 5, from 5 to 7, and from 7 back to 2. Dealing into 5 or into 8 piles will make three circuits each from one vertex to the next one of 3 squares inscribed in the circle. Dealing into 3 or into 9 piles will give 4 circuits round three inscribed equilateral triangles. Finally, dealing into 12 piles, with regathering, etc. according to rule, simply reverses the order so that the ace and queen, the 2 and knave, the 3 and ten, etc. change places.

It has already been made evident that if any prime number, P, of cards, each inscribed with a number, so that, when operations begin this number shall be equal to the ordinal place of the card in the pack, be dealt into any lesser number, S, of piles, and these be re-gathered, etc. according to rule, the effect is cyclically to multiply by S the number inscribed on any card which is identified solely by its resulting ordinal place, that is, to multiply in counting the numbers round and round a cycle of P numbers,—or, to state it otherwise, the ordinary product has the highest lesser multiple of P subtracted from it, though this seems to me to be a needlessly complicated form of conceiving the cyclical product. In counting round and round, the number of numbers in the cycle, the so-called "modulus of the cycle," is the same as zero; so that the product of its multiplication by S is zero; or, regarding the matter in the other way, SP diminished by the largest lesser multiple of P gives P. Consequently, the face card will not change its face-value. Let the dealing etc. be reiterated until it has been performed δ times. The effect will be to multiply the face-values (of cards identified only by their final ordinal places) by S^δ. Since this is the same multiplier for all the cards, it follows that when δ attains such a value that the card in any one place, with the exception of the face card of the pack, *which alone retains an unchanging value,* recovers its original value, every one of the P–1 cards of (apparently) changing values equally recovers its original value; and if the values do not shift round a single circuit of P–1 cards, all the circuits must be equal; for otherwise the single number S^δ would not fix the values of all the cards. And since zero, or P, is the only number that remains unchanged by a multiplication where the multiplier is not unity (and S is always cyclically greater, that is, more advanced clockwise, than 1 and less than P,) it follows that the moduli of the shifts must all be the same divisor of P–1, and consequently P–1 deals, whatever be the constant number of piles, must restore the original order. The pure arithmetical statement of this result is that S^{P-1}, whenever P is a prime number and S not a multiple of it, must exceed by one some multiple of P. This proposition goes by the name of its discoverer, perhaps the most penetrating mind in the history of mathematics; being known as "Fermat's theorem"; although from our present point of view, it may seem too obvious to be entitled to rank as a "theorem." The books give half a dozen demonstrations of it. It lies at the root of cyclic arithmetic.

Fermat said he possessed a demonstration of his theorem; and

there is every reason for believing him; but he did not publish any proof. About 1750, the mathematician König asserted that he held an autograph manuscript of Leibniz containing a proof of the proposition; but it has never been published, so far as I know. Euler, at any rate, first published a proof of it; and Lambert gave a similar one in 1769. Subsequently Euler gave a proof less encumbered with irrelevant considerations; and this second proof is substantially the same as that in Gauss's celebrated "*Disquisitiones Arithmeticae*" of 1801, §49. Several other simple proofs have since been given; but none, I think, better than that derived from the consideration of repeated deals.

But what concerns the curious phenomenon of my little "trick" is not so much Fermat's theorem as it is the more comprehensive fact that, whatever odd prime number, P, the number of cards in the pack may be, there is some number, S, such that in repeated deals into that number of piles, all the numbers less than P shift round a single circuit. I hope and trust, Reader, that you will not take my word for this. If fifty years spent chiefly with books makes my counsel about reading of any value, I would submit for your approbation the following maxims:

I. There are more books that are really worth reading than you will ever be able to read. Confine yourself, therefore, to books worth reading and re-reading; and as far as you can, own the good books that are valuable to you.

II. Always read every book critically. A book may have three kinds of value. First, it may enrich your ideas with the mere possibilities, the mere ideas, that it suggests. Secondly, it may inform you of facts. Thirdly, it may submit, for your approbation, lines of thought and evidences of the reasonable connection of possibilities and facts. Consider carefully the attractiveness of the ideas, the credibility of the assertions, and the strengths of the arguments, and set down your well-matured objections in the margins of your own books.

III. Moreover, procure, in lots of twenty thousand or more, slips of stiff paper of the size of postcards, made up into pads of fifty or so. Have a pad always about you, and note upon one of them anything worthy of note, the subject being stated at the top and reference being made below to available books or to your own note books. If your mind is active, a day will seldom pass when you do not find a dozen items worth such recording; and at the end of twenty years, the slips having been classified and arranged and

rearranged, from time to time, you will find yourself in possession of an encyclopædia adapted to your own special wants. It is especially the small points that are thus to be noted; for the large ideas you will carry in your head.

If you are the sort of person to whom anything like this recommends itself, you will want to know what evidence there is of the truth of what I assert, that there is some number of piles into which any prime number of cards must be dealt out one less than that prime number of times before they return to their original order.

If these maxims meet your approval, and you read this screed at all, you will certainly desire to see my proposition proved. At any rate, I shall assume that such is your desire. Very well; proofs can be found in all the books on the subject from the date of Gauss's immortal work down. But all those proofs appear to me to be needlessly involved, and I shall endeavor to proceed in a more straightforward way, which "mehr rechnend zu Werke geht." Indeed, I think I shall render the matter more comprehensible by first examining a few special cases. But at the outset let us state distinctly what it is that is to be proved. It is that if P is any prime number greater than 2, then there must be some number of piles, S, into which a pack of P cards must be dealt (and regathered and cut, according to the rule) P–1 times in order to bring them all round to their original places again. The reason I limit the proposition to primes will presently appear: the reason I limit the primes to those that are greater than 2 is that two cards cannot, in accordance with the rule be dealt, etc., into more than one pile (if you call that dealing); and of course this does not alter the arrangement; and since there is no number of piles less than one, the theorem, in this case, reduces itself to an identical proposition; while if 1 be considered to be a prime number, the proposition is falsified since there is no number of piles into which one card can be dealt and regathered according to the rule, which requires S to be less than P.

Let our first example be that of P=17. Then P–1=16; and unless there be a single circuit of 16 face-values, which my whole present object is to show that there must be, all the circuits must either be one or more sets of 8 circuits of 2 values each, or sets of 4 circuits of 4 values each, or sets of 2 circuits of 8 values each; unless, indeed, we count in, as we ought to do, the case of 16 circuits of 1 value each. This last means that each of the 16 cards retains its face-value after a single deal. It is obtrusively obvious that this can only be when S=1. But since in these hints toward a demonstra-

tion of the proposition the particular values of S do not concern us, and had better be dismissed from our minds, we will denote this value of S by S^{xvi}, meaning that it is a value that gives 16 circuits. We will now ask what is the number of piles into which 2 dealings will restore the face-value of every card; or, in other words, will give 8 circuits of 2 values each. Letting x denote that unknown quantity, the number of piles, or the cyclic multiplier, the equation to determine it is $x^2=1$. To many readers two values satisfying this equation will be apparent. But I do not care what they are, further than that the value $x=1$ obviously satisfies the equation $x^2=1$. I do care, however, to show that there can be but two solutions of the equation $x^2=1$. For suppose that $x_1^2=1$ and $x_2^2=1$. Then $x_2^1-x_2^2=(x_1+x_2)\cdot(x_1-x_2)=0$ or equals mP. Now if a multiple of a prime number be separated into two or more factors, one of these, at least, must itself be a multiple of that prime, just as in the algebra of real and of imaginary quantities and in quaternions, if the product of several quantities be zero, one or other of those factors must be zero; and just as in logic, if an assertion consisting of a number of asserted items be false, one or more of these items must be false. In addition, every summand has its own independent effect; but every unit of a product is compounded of units of all the several factors. This is the formal, or purely intellectual, principle at the root of all the reasons for making the number of cards dealt, especially in reiterated dealings, to be a prime. It follows, then, that there are but two numbers of piles dealings into each of which will restore the original arrangement after 2 deals; and one of these is $x=1$; for evidently (bear this in mind,) if $x^a=1$, then also $x^{(ab)}=(x^a)^b=1$. There is then but one number of piles dealings into which shift the values of the cards in eight, and only eight, circuits; and this number we will denote by S^{viii}. Then, reserving x to denote any root of the equation $x^2=1$, and taking ξ to denote that one of the two roots that is not 1, we will take y to denote any number of piles, after dealing into which 4 times, the resulting arrangement of the values will be the original arrangement. That is to say, y will be any root of the cyclic equation $y^4=1$. But $x^4=(x^2)^2=1^2=1$; so that any value of x is a value of y. Let η denote any value of y that is not a value of x; and let us suppose that there are two values of η, which we may denote by $S^{iv}$and S^{xii}. It will be easy to show that there is no third value of η. For $(\eta^2)^2=1$, where η^2 fulfills the definition of x and is thus either 1 or ξ. But the roots of the equation $\eta^2=1$ fulfill the definition of x, whose values are excluded from the

definition of η. Hence we can only have $\eta^2=\xi$; and that this has but two roots is proved by the same argument as was used above. Namely, η_1 and η_2 being any two of these, $(\eta_1^2-\eta_2^2)=(\eta_1+\eta_2)\cdot(\eta_1-\eta_2)=0$, so that unless η_1 and η_2 are equal, and $\eta_1-\eta_2=0_1$, then $\eta_1+\eta_2=0$ or η_1 and η_2 are negatives of each other. Now no more than 2 quantities can be each the negative of each of the others. We now pass to the consideration of those numbers of piles into which eight successive dealings result in the original arrangement. Denoting by z any such number, it is defined by the equation $z^8=1$. But every value of y (of which we have seen that there cannot be more than 4,) satisfies this equation, since $y^8=(y^4)^2=1^2=1$. Let ζ denote any value of z which is not a value of y. We may suppose that there are two of these for each of the two values of η, which we will designate as S^{ii}, S^{vi}, S^{x}, S^{xiv}. I need not assert that there are so many; but my argument requires me to prove that there are no more. The equation $(z^2)^4=z^8=1$ shows that z^2 fulfills the definition of y and can therefore have no more than the four values 1, ξ, and the two values of η. Now if $z^2=1$, z can, as we have seen in the case of x, have no other values than $z=1$ and $z=\xi$, both of which are values of y.

If $z^2=\xi$, as we have seen in regard to y, z can have no other values than the two values of η, which are again values of y. Now let us suppose that z has four values, S^{ii}, S^{vi}, S^{x}, and S^{xiv}, that are not values of y; and let us define ζ as any value of z that is not a y. The proof that there can be no more than four ζs is so exactly like the foregoing as to be hardly worth giving. I will relegate it to a paragraph of its own that shall be both eusceptic and euskiptatic;—"what horrors!' I hear from the mouths of those moderns who abominate all manufactures of Hellenic raw materials, like "skip" and "skimp."

We have seen that either $z^2=1$, or $z^2=\xi$, or $z^2=\eta$; and also that, in the first case, either $z=1$ or $z=\xi$, both of which are values of y; and that, in the second case, z has one or other of the two values of η. Accordingly, it only remains that $\zeta^2=\eta$. There are but two values of η and if ζ_1 and ζ_2 are two different values of ζ whose squares are the same value of η, $\zeta_1^2-\zeta_2^2=(\zeta_1+\zeta_2)\cdot(\zeta_1-\zeta_2)=0$. Hence, since $\zeta_1-\zeta_2$ is not zero, it follows that every value of ζ differs from every other value derived from the same η only by being the negative of it. Now no number has two different negatives; and therefore there can be no more than two ζs to every η; and there being no more than two ηs, there can be no more than four ζs.

Now this is the summary of the whole argument: the 17 cards of the pack being consecutively inscribed with numbers from the back to the face of the pack, each number of piles into which they are dealt etc. according to the rule acts as a cyclic multiplier of the face-value of every card. Every such multiplier leaves 0(=17) unchanged, and shifts the other 16 face-values in a number of circuits having the same number of values in each. The possible consequences, excluding the case of a single circuit of 16 values, are the following:

16 circuits of 1 value each can result from but	1	multiplier at the utmost.
8 circuits of 2 values each can result but from	1	other multiplier
4 circuits of 4 values each can result but from	2	other multipliers
2 circuits of 8 values each can result but from	4	other multipliers
In all the number of multipliers that give more than 1 circuit (of all 16 values) is.....	8	at most
But there are in all	16	multipliers
Hence, the number of multipliers that shift the values in 1 circuit of 16 values is.........	8,	at least.

In point of fact, it is precisely 8.

Let us now consider a pack of 31 cards. Here, the zero card not changing its value, there are 30 values which are shifted in one of these ways:

In 30 circuits of 1 value each;
In 15 circuits of 2 values each;
In 10 circuits of 3 values each;
In 6 circuits of 5 values each;
In 5 circuits of 6 values each;
In 3 circuits of 10 values each;
In 2 circuits of 15 values each;
In 1 circuit of 30 values.

I propose to show as before that if we exclude the last case, the others do not account for the effects of so many as 30 different multipliers. In the first place, as in the last example, but one multiplier will give circuits of one value each; and but one other multiple will give circuits of only two values each. We may call the former S^{xxx} and the latter S^{xv}.

The problems of 10 circuits of 3 values each and of 6 circuits of 5 values each can be treated by exactly the same method, 3 and

5 being prime numbers. I shall exhibit in full the solution of the more complicated of the two, leaving the other to the reader.

I propose, then, to show that there are at most but 5 different values which satisfy an equation of the form $s^5=1$. The general idea of my proof will be to assume that there are 5 different values (for it is indifferent to my purpose whether there be so many or not,) and then to show that there is such an equation between these five, that given any four, there is but one value that the fifth can have; that being as much as to say that there are not more than five such values in all. This assumes that every one of the five values differs from every one of the other four; making ten premisses of this kind that have to be introduced. Now to introduce a premiss into a reasoning, is to make some inference which would not necessarily follow if that premiss were not true. Assuming, then, that $s^5=1$, $t^5=1$, $u^5=1$, $v^5=1$, $w^5=1$, are the five assumed equations, I note that the division by one divisor of both sides of an equation necessarily yields equal quotients only if the divisor is known not to be zero. Hence if I divide my equations by $s-t$, by $s-u$, by $s-v$, by $s-w$, by $t-u$, by $t-v$, by $t-w$, by $u-v$, $u-w$, and by $v-w$, I shall certainly introduce the ten premisses that all the five values are different; and with a little ingenuity,—a *very* little, as it turns out,—I ought to reach my legitimate conclusion.

I will begin then by subtracting $t^5=1$ from $s^5=1$, giving $s^5-t^5=0$; and dividing this by $s-t$, and using $\cdot|\cdot$ as the logical sign of disjunction, that is, to mean "or else," I get

$$(1)\quad s^4+s^3t+s^2t^2+st^3+t^4=0 \;\cdot|\cdot\; s=t.$$

By analogy, I can equally write

$$s^4+s^3u+s^2u^2+su^3+u^4=0 \;\cdot|\cdot\; s=u.$$

Subtracting the latter of these from the former, I get,

$$s^3(t-u)+s^2(t^2-u^2)+s(t^3-u^3)+t^4-u^4=0 \;\cdot|\cdot\; s=t \;\cdot|\cdot\; s=u.$$

And dividing this by $t-u$, I obtain

$$(2)\quad s^3+s^2(t+u)+s(t^2+tu+u^2)+t^3+t^2u+tu^2+u^3=0 \;\cdot|\cdot\; s=t \;\cdot|\cdot\; s=u \;\cdot|\cdot\; t=u.$$

By analogy, I can equally write

$$s^3+s^2(t+v)+s(t^2+tv+v^2)+t^3+t^2v+tv^2+v^3=0 \;\cdot|\cdot\; s=t \;\cdot|\cdot\; s=v \;\cdot|\cdot\; t=v.$$

Subtracting the last equation from the last but one, I get

$$(s^2+st+t^2)(u-v)+(s+t)(u^2-v^2)+u^3-v^3=0 \;\cdot|\cdot\; s=t \;\cdot|\cdot\; s=u \;\cdot|\cdot\; s=v \;\cdot|\cdot\; t=u \;\cdot|\cdot\; t=v.$$

and dividing by $u-v$, I have

$$(3)\quad s^2+st+t^2+(s+t)(u+v)+u^2+uv+v^2=0 \;\cdot|\cdot\; s=t \;\cdot|\cdot\; s=u \;\cdot|\cdot\; s=v \;\cdot|\cdot\; t=u \;\cdot|\cdot\; t=v \;\cdot|\cdot\; u=v.$$

By analogy, I can equally write

$$s^2+st+t^2+(s+t)(u+w)+u^2+uw+w^2=0 \cdot|\cdot s=t \cdot|\cdot s=u \cdot|\cdot s=w \cdot|\cdot t=u \cdot|\cdot t=w \cdot|\cdot u=w.$$

Subtracting the last from the last but one, and dividing by $v-w$, I get

$$(4) \quad s+t+u+v+w=0 \cdot|\cdot s=t \cdot|\cdot s=u \cdot|\cdot s=v \cdot|\cdot s=w \cdot|\cdot t=u \cdot|\cdot t=v \cdot|\cdot t=w \cdot|\cdot u=v \cdot|\cdot u=w \cdot|\cdot v=w.$$

This shows at once that there cannot be more than 5 different numbers, which, counting round any prime cycle, all have their 5th powers equal to 1. By a similar process, as you can almost see without slate and pencil, from $x^3=1$, $y^3=1$, $z^3=1$ one can deduce $x+y+z=0 \cdot|\cdot x=y \cdot|\cdot x=z \cdot|\cdot y=z$. The existence of these 5 and these 3 numbers must, for the present, be regarded as problematic, except that we cannot shut our eyes to the fact that 1 is one of the members of each set; as indeed $1^\delta=1$, whatever the exponent may be.

I have numbered some of the equations obtained in the proof that there are no more than 5 fifth roots of unity. You will observe that (1) equates to zero the sum of all possible terms of the fourth degree formed by two roots; that (2) equates to zero the sum of all possible terms of the third degree formed by three roots; that (3) equates to zero the sum of all possible terms of the second degree formed from four roots; and that (4) equates to zero the sum of all possible terms of the first degree formed by all five roots. Now it is plain that if we assume that there are n unequal nth roots of unity, then by subtracting $x_2^n=1$ from $x_1^n=1$, and dividing by x_1-x_2, we shall equate to zero the sum of all possible terms of the $(n-1)$th degree in x_1 and x_2. And if we have proved, in regard to any m of the roots, that (all being unequal,) the sum of all possible terms of the $(n-m+1)$th degree in these roots is equal to zero; then by taking two such equations of the $(n-m+1)$th degree in $m-1$ roots common to the two, with one root in each equation not entering into the other; by subtracting one of these equations from the other, and then dividing by the difference between the two roots which enter each into but one of these equations, we shall get an equation of the $(n-m)$th degree in $m+1$ roots. For $x^n-y^n=(x-y)\cdot\sum_{i=0}^{n-1} x^i y^{n-i-1}$

Accordingly, by repetitions of this process, we shall ultimately find that the sum of the n roots, if there be so many, is 0. This proves that there can be no more than n unequal nth roots of unity in cyclic arithmetic any more than in unlimited real or imaginary arithmetic.

But if the root of unity be of an order not prime but composite, so that it is the root of an equation of the form $x^{pq}=1$, it is evident that it is satisfied by every root of $y^p=1$ and by every root of $y^q=1$; since every power of 1 is 1. Accordingly, exclusive of roots of a lower order, the number of roots of unity of order n, that is, the number of roots of $x^n=1$, additional to those that are roots of unity of lower order cannot be greater than the number of numbers not greater than n and prime to it. A number is said to be prime to a number when they have no other common divisor than 1. I shall write the expression of two or more numbers separated by heavy vertical lines to denote the greatest common divisor of those numbers. Thus, I shall write $12\mid18=6$. This vertical line may be considered as a reminiscence of the line that separates numbers in the usual algorithm of the greatest common divisor. A prime number is a number prime to every other number. Consequently, 1 is a prime number. It is the only prime number that is prime to itself; for $p\mid p=p$. The number of numbers not exceeding a number, n, but prime to it is now called the *totient* of n. In the books of the first four fifths of the nineteenth century, the totient of n was denoted by $\phi(n)$; but since the invention of the word *totient*, about 1880, Tn has become the preferable notation. T1=1; but if p be a prime not prime to itself T$p=p-1$. It is quite obvious that the totient of any number, n, whose prime factors not prime to themselves are p', p'', p''', etc. is obtained by subtracting from n the p'th part of it, and then successively from each remainder the p''th, etc. part of it, but not using any prime factor twice. Thus T4=2 (for $4\mid1=1$ and $4\mid3=1$; but $4\mid2=2$ and $4\mid4=4$); T6=2 (for $6-\frac{1}{2}\cdot6=3$ and $3-\frac{1}{3}3=2$); T8=4 (for $8-\frac{1}{2}\cdot8=4$), T9=6, T10=4, etc. If $m\mid n=1$, then Tmn=(Tm)(Tn). On the other hand, if p is a prime and m any exponent, T$p^n=(p-1)p^{n-1}$. A "perfect number" is defined as one which is equal to the sum of its "aliquot parts," that is, of all its divisors except itself; but, in a more philosophical sense, *every* number is a perfect number. That is to say, it is equal to the sum of the totients of *all* its divisors;—a proposition which is perfectly obvious if regarded from the proper point of view. However, since this proposition has some relevancy to the proposition I am endeavoring to prove; namely, that there is some number of piles, dealing into which shifts all the face-values of the cards along a single cycle, I will repeat a pretty demonstration of the former proposition that I find in the books. Having selected any number, m, rule a sheet of paper into columns, a column for each divisor

of m; and write these divisors, in increasing order from left to right each at the top of its column as its principal heading. Just beneath this, write in parentheses, as a subsidiary heading to the column, the complementary divisor, i. e., the divisor whose product into the principal heading is the number m; and draw a line under this subsidiary heading. Now, to fill up the columns, run over all the numbers in regular succession, from 1 up to m inclusive, writing each in one column, and in one only; namely in that column which is furthest to the right of all the columns of whose principal headings the number to be written is a multiple. Here, for example, is the table for m=20:

1 (20)	**2** (10)	**4** (5)	**5** (4)	**10** (2)	**20** (1)
1	2				
3		4	5		
	6				
7		8			
9				10	
11		12			
13	14		15		
		16			
17	18				
19					20

By this means it is obvious that each column will receive all those multiples of the principal heading whose quotients by that heading are prime to the subsidiary heading, and will receive no other numbers. Thus, every column will contain just one number for each number prime to the subsidiary heading but not greater than it; [since no number is entered which exceeds the product of the two headings.] In other words, the number of numbers in each column equals the totient of the subsidiary heading; and since the subsidiary headings are all the divisors, and the total number of numbers entered is m, the sum of the totients of all the divisors of m is m, whatever number m may be. It will be convenient to have a name for this principle; and since, as I remarked, it renders every number a perfect number in a perfected sense of that term, or say a *perfecti perfect* number, I will refer to it as the *rule of perfection.*

According to this, although $x^6=1$ may have 6 roots, yet since x^2, x^3, and x^6 are also roots, by the rule of perfection there can be but $T6=T2\cdot T3=1\cdot 2=2$ numbers of piles into which dealing must

be made 6 times successively in order to restore the original arrangement; and similarly for the other divisors. So then the number of ways of dealing (i. e., number of piles into which the cards can be dealt, etc.) which will restore 31 cards to their original order in less than 30 deals cannot exceed T1+T2+T3+T5+T6+T10+T15. There are, however, in all 30 ways of dealing; and by the rule of perfection 30=T1+T2+T3+T5+T6+T10+T15+T30. Hence, there must be T30=T2 · T3 · T5=1 · 2 · 4=8 ways of dealing which shift the 30 values in a single circuit. And so with any other prime number than 31. This argument is so near a perfect demonstration that there always must be such ways of dealing that I may leave its perfectionment to the reader.

I do not know of any general rule for ascertaining what the particular numbers of piles are into which the prime number p of cards must be dealt $p-1$ times in order to bring round the original arrangement again. It seems that there is a *Canon Arithmeticus* got out by Jacobi, which gives the numbers for the first 170 primes or so. It was published in the year of my birth; so that it was clearly the purpose of the Eternal that I should have the advantage of it. But that purpose must have been frustrated; for I never saw the book. The *Tables Arithmétiques* of Hoüel (Gauthiers-Villars: 1866. 8vo, pp. 44) gives those numbers for all primes less than 200. From these tables it appears that for about five-eighths of the primes one such number is either 2 or $p-2$. Now as soon as one has been found, it is easy to find the rest which are all the powers of that one whose exponents are prime to $p-1$. In case $p-1$ has few prime factors, the numbers any one of which we seek must be nearly a third, perhaps nearly or quite half of all the $p-1$ numbers; so that ere many trials have been made, one is likely to light upon one of them. Thus if $p=17$, try 2. Now $2^4=16=-1$; so this will not do. Nor will -2. Try 3. We have $3^2=9=-8$; $3^3=27=-7$, $3^4=81=-4$, $3^8=(3^4)^2=(-4)^2=16=-1$. Evidently 3 is one of the numbers and the others are $3^3=-7$, $3^5=-12=5$, $3^7=(3^3)(3^4)=(-7)\cdot(-4)=28=-6$, and the negatives of these. If the prime factors are many, a different procedure may be preferable. Take the case of $p=31$. Here $p-1=2\cdot 3\cdot 5$. Turning to that table of the first nine powers of the first hundred numbers which is given in so many editions of Vega, I find in the column of cubes, $5^3=125=4(31)+1$, and $6^3=216=7\cdot 31-1$; and in the column of 5th powers, I find $3^5=243=8(31)-5$. Consequently, $(3^5)^3=3^{15}=-1$. This renders it *likely* that 3 may be such a number as I seek. $3^2=9$, $3^3=-4$, $3^4=-12$, $3^5=-5$, $3^6=16=-15$, $3^{10}=-6$, $3^{12}=+8$, 3^{15}

$=(3^5)^3=-125=1$. It is evident that 3 is one of the numbers. The other seven are $3^7=3^5\cdot 3^2=-45=-14$, $3^{11}=3\cdot 3^{10}=-18=13$, $3^{13}=3\cdot 3^{12}=24=-7$, $3^{17}=3^{15}\cdot 3^2=-9$; $3^{19}=3^{15}\cdot 3^4=+12$, $3^{23}=3^{19}\cdot 3^4=-144=+11$, $3^{29}=3^{17}\cdot 3^6=(-9)\cdot(-15)-135=+11$.

Since, then, whatever prime number not prime to itself p may be, there are always $T(p-1)$ numbers of which the lowest power equal to 1 (counting round the p cycle,) is the $(p-1)$th and these powers run through all the values of the cycle excepting only $p=0$, it follows that these numbers may appropriately be called *basal* (or *primitive*) roots of the cycle; and that their exponents are true *cyclic logarithms* of all the numbers of the cycle except zero. But since, if b be such a basal root, its $(p-1)$th power, like that of any other number, equals 1 (counting round the p-cycle), it follows that these exponents run round a cycle smaller by one unit than that of their powers; or in other words, the *modulus* of the cycle of logarithms is $p-1$, while the modulus of the cycle of natural numbers is p.*

The cyclic logarithms form an entirely distinct number-system from that of the corresponding natural numbers. For the modulus

* This being the first occasion I have had in this essay to employ the word "modulus," I will take occasion to say that its general meaning is now well established. It means that signless quantity which measures the magnitude of a quantity and is a factor of it. So that if M and M′ are the moduli of two quantities, $M\mu$ and $M'\mu'$, their product is $MM'\cdot\mu\mu'$, where MM′ is an ordinary product, but $\mu\mu'$ may be a peculiar function. Thus, the absolute value of -2, or 2, is its "modulus," as 3 is of -3; and $(-2)\cdot(-3)=+6$ where $2\times 3=6$ by ordinary multiplication, but $(-1)\times(-1)=+1$ by an extension of ordinary multiplication. So the "modulus" of $A+Bi$, where $i^2=-1$, is $\sqrt{A^2+B^2}$. The tensor of a quaternion and the determinant of a square matrix are other examples of moduli. The cardinal number of numbers in a cycle has no sign and may properly be called the modulus of the cycle. But I sometimes refer to it as "the cycle," for short. The present usage of mathematicians is to use, what seems to me a too involved way of conceiving of cyclic arithmetic which carries with it an irregular use of the word "modulus." Legendre and the earlier writers on cyclic arithmetic conceived of its numbers as signifying the lengths of different steps along a cycle of objects, and thus spoke of 18 as being *equal* to 1 on a cycle of 17, just as we say that the 1st, 15th, 22d, and 29th days of August fall on *the same* day of the week, and just as we say that 270° of longitude west of any meridian and 90° east of it are *the very same* longitude. Gauss, however, introduced a different locution, involving quite another form of thought. Instead of saying that 18 *is*, or *equals*, 1 in counting round a cycle of modulus 17, he prefers to say that 18 and 1 belong to the same *class* of numbers *congruent to* one another for the *modulus* 17. Here the idea of a cycle appears to be rejected in favor of the idea that $(18-1)/17$ is a whole number.

Now I fully admit that the conception of an indefinitely advancing series is involved in that of a cycle, and further that non-cyclical numbers have to be used to some extent in cyclic arithmetic. But at the same time it seems to me that the theoric idea of a cycle ought to take the lead in this branch of mathematics. In particular, I cannot see why the term *cyclic logarithms* is not perfectly correct and far more expressive than Gauss's colorless name of "indices."

of their cycle is composite instead of prime, a circumstance which essentially modifies some of the principles of arithmetic. For example, every natural number of a cycle of prime modulus gives an unequivocal quotient when divided by another. But some numbers in a cycle of composite modulus give two or more quotients when divided by certain others, while others are not divisible without remainders. The whole doctrine shall be set forth here. I will preface it with a statement of the essential differences between the system of all positive finite integers, the system of all real finite integers, and any cyclical system. I omit the Cantorian system, partly because the full explanation of it would be needed and would be long, and partly because there is a doubt whether it really possesses an important character which Cantor attributes to it.

It is singular that though the systems to be defined possess, besides several independent common characters, others in respect to which they differ, yet *all* the properties of each system are necessary consequences of a single principle of immediate sequence. In stating this, I shall abbreviate a frequently recurring phrase of nine syllables by writing, '*m* is A of (or to) *n*,' or even '*m* is A*n*,' to mean that the member, *m*, of the system is in a certain relation of immediate antecedence to the member *n*. I shall express the same thing by writing '*n* is A'd by *m*.' But when I call A an abbreviation, I do not mean to imply that the words "immediately antecedent" express its meaning in a satisfactory way. On the contrary, in part, they suggest something repugnant to its meaning, which must be gathered exclusively from the following definitions of the three kinds of systems:

A *cyclical system* of objects is such a collection of objects that, the expression '*m* is A to *n*' signifying some recognizable relation of *m* to *n*, every member of the system is A to some member or other, and whatever predicate, P, may be, if P is true of no member of the system without being true of some member of it that is A'd by that member, then P is true either of no member or of every member.

The system of all positive whole numbers is a single collection of numbers, the general essential character of which collection is that there is a recognizable relation signified by A, such that every positive integer is A to a positive integer, and there is one, and one only, initial positive integer, 0 (or, if this be excluded, then 1,) such that, whatever predicate P may be, if P is true of no positive integer without being also true of some positive integer to which

the former is A, then either this predicate is false of that initial positive integer or else is true of all positive integers.

The system of all real integers is a collection of numbers of which the general essential character is that there is recognizable relation signified by one being A to another, such that every number of the system is both A to a number of the system, and is A'd by a number of the system, and whatever predicate P may be, if this be not true of any number, n, of the system without being both true of some number that is A of n, and true also of some number that is A'd by n, then P is either false of every number of the system or is true of every number of the system.

A *Cantorian* system is essentially a system of objects positively determined by every collection of objects of the system being A to some object of the system, and by a certain object, 0, being a member of the system; while it is negatively determined by the principle that, whatsoever predicate P may be, if P is not true of every member of any collection of the system without being also true of some member that is A'd by that collection, then either P is not true of the member, 0, or it is true of every member of the system.

Now for several reasons, partly for the sake of the logical interest and instruction that will accrue I will proceed to show precisely *how* all the fundamental properties common to cyclical systems follow from my definition. In accordance with the usage of logicians and mathematicians, I shall call this "demonstrating" those properties. The reader must not fall into the error of supposing that, by this expression, I mean *rationally convincing* him that all cyclical systems have these properties; for I know well that he is perfectly cognizant of that already. All I am seeking to convince him of is, 1st, *that,* and 2d, *how,* their truth of all cyclical systems follows from my definition. But in the course of doing so, I shall endeavor to bring to his notice some things well worth knowing concerning necessary reasonings in general. Especially, I shall try to point out errors of logical doctrine which students of the subject who neglect the logic of relations are apt to fall into.

A brace of these errors, are, first, that nothing of importance can be deduced from a single premiss; and secondly, that from two premisses one sole complete conclusion can be drawn. Persons who hold the latter notion cannot have duly considered the paucity of the premisses of arithmetic and the immensity of higher arithmetic, otherwise called the "theory of numbers," itself. As to the former belief, aside from the consideration that whatever follows

from two propositions equally follows from the one which results from their copulation, they will have occasion to change their opinion when they come to see what can be deduced from the definition of a cyclic system, which definition is not a copulative proposition.

That couple of logical heresies, being married together, legitimately generates a third more malignant than either; namely, that necessary reasoning takes a course from which it can no more deviate than a good machine can deviate from its proper way of action, and that its future work might conceivably be left to a machine,—some Babbage's analytical engine or some logical machine (of which several have actually been constructed). Even the logic of relations fails to eradicate that notion completely, although it does show that much unexpected truth may often be brought to light by the repeated reintroduction of a premiss already employed; and in fact, this proceeding is carried to great lengths in the development of any considerable branch of mathematics. Although, moreover, the logic of relations shows that the introduction of abstractions,—which nominalists have taken such delight in ridiculing,—is of the greatest service in necessary inference, and further shows that, apart from either of those manoeuvres,—either the iteration of premisses or the introduction of abstractions,—the situations in which the necessary reasoner finds several lines of reasoning open to him are frequent. Nevertheless, in spite of all this, the tendency of the logic of relations itself,—the highest and most rational theory of necessary reasoning yet developed,—is to insinuate the idea that in necessary reasoning one is always limited to a narrow choice between quasi-mechanical processes; so that little room is left for the exercise of invention. Even the great mathematician, Sylvester, perhaps the mind the most exuberant in original ideas of pure mathematics of any since Gauss, was infected with this error; and consequently, conscious of his own inventive power, was led to preface his "Outline Trace of the Theory of Reducible Cyclodes," with a footnote which seems to mean that mathematical conclusions are not always derived by an apodictic procedure of reason. If he meant that a man might, by a happy guess, light upon a truth which might have been made a mathematical conclusion, what he said was a truism. If he meant that the hint of the way of solving a mathematical problem might be derived from any sort of accidental experience, it was equally a matter of course. But the truth is that all genuine mathematical work, except the formation of the initial postulates (if this be regarded as mathematical

work,) is necessary reasoning. The mistake of Sylvester and of all who think that necessary reasoning leaves no room for originality,—it is hardly credible however that there is anybody who does not know that mathematics calls for the profoundest invention, the most athletic imagination, and for a power of generalization in comparison to whose every-day performances the most vaunted performances of metaphysical, biological, and cosmological philosophers in this line seem simply puny,—their error, the key of the paradox which they overlook, is that originality is not an attribute of the *matter* of life, present in the whole only so far as it is present in the smallest parts, but is an affair of *form,* of the way in which parts none of which possess it are joined together. Every action of Napoleon was such as a treatise on physiology ought to describe. He walked, ate, slept, worked in his study, rode his horse, talked to his fellows, just as every other man does. But he combined those elements into shapes that have not been matched in modern times. Those who dispute about Free-Will and Necessity commit a similar oversight. Notwithstanding my tychism, I do not believe there is enough of the ingredient of pure chance now left in the universe to account at all for the indisputable fact that mind acts upon matter. I do not believe there is any amount of *immediate* action of that kind sufficient to show itself in any easily discerned way. But one endless series of mental events may be immediately followed by a beginningless series of physical transformations. If, for example, all atoms are vortices in a fluid, and every fluid is composed of atoms, and these are vortices in an underlying fluid, we can imagine one way in which a beginningless series of transformations of energy* might take place in a fraction of a second. Now whether this particular way of solving the paradox happens to be the actual way, or not, it suffices to show us that from the supposed fact that mind acts *immediately* only on mind, and matter *immediately* only on matter, it by no means follows that mind cannot act on matter, and matter on mind, without any *tertium quid.* At any rate, our power of self-control certainly does not reside in the smallest bits of our conduct, but is an effect of building up a character. All supremacy of mind is of the nature of Form.

The plan of a demonstration can obviously not spring up in the mind complete at the outset; since when the plan is perfected, the

* You may well be puzzled, dear Reader, to iconize the consecution of a beginningless series upon an endless series. But you have only to imagine a dot to be placed upon the rim of a half-circle at each point whose angular distance from the beginning of the semicircumference has a positive or nega-

demonstration itself is so. The thought of the plan begins with an act of ἀγχίνοια* which, in consequence of pre-existent associations, brings out the idea of a possible object, this idea not being itself involved in the proposition to be proved. In this idea is discerned that the possibility of its object follows in some way from the condition, general subject, or antecedent of the proposition to be proved, while the known characters of the object of the new idea will, it is perceived, be at least adjuvant to the establishment of the predicate or consequent of that proposition.

I shall term the step of so introducing into a demonstration a new idea not explicitly or directly contained in the premisses of the reasoning or in the condition of the proposition which gets proved by the aid of this introduction, a *theō'ric* step. Two considerable advantages may be expected from such a step besides the demonstration of the proposition itself. In the first place, since it is a part of my definition that it really aids the demonstration, it follows that without some such step the demonstration could not have been effected, or at any rate only in some very peculiar way. Now to propositions which can only be proved by the aid of theoric

tive whole number for its natural tangent. These dots will, then, occur at the following angular distances from the origin of measurement.

ANGULAR DISTANCE	TANGENT	ANGULAR DISTANCE	TANGENT	ANGULAR DISTANCE	
0° 00′	0	87° 24′	+22	93° 01′	—19
45 00	+ 1	87 31	+23	93 11	—18
63 26	+ 2	87 37	+24	93 21	—17
71 34	+ 3	87 43	+25	93 35	—16
75 58	+ 4			93 49	—15
78 41	+ 5	and so on endlessly. But		94 05	—14
80 32	+ 6	after all positive integer		94 24	—13
81 52	+ 7	values have been passed		94 46	—12
82 52	+ 8	through before 90°,		95 12	—11
83 40	+ 9	(where there will not be		95 43	—10
84 17	+10	any dot,) a beginningless		96 20	— 9
84 48	+11	series of dots will suc-		97 08	— 8
85 14	+12	ceed, for which the tan-		98 08	— 7
85 36	+13	gents are negative; and		99 28	— 6
85 55	+14	then		101 19	— 5
86 11	+15			104 02	— 4
86 25	+16	92° 17′	—25	108 26	— 3
86 38	+17	92 23	—24	116 34	— 2
86 49	+18	92 29	—23	135 09	— 1
86 59	+19	92 36	—22	180 00	0
87 08	+20	92 44	—21	225 00	+ 1
87 16	+21	92 52	—20	etc.	

* See *Charmides,* p. 160A, and the last chapter of the First *Posterior Analytics.*

steps, (or which, at any rate, could *hardly* otherwise be proved,) I propose to restrict the application of the hitherto vague word "*theorem*," calling all others, which are deducible from their premisses by the general principles of logic, by the name of *corollaries*. A theorem, in this sense, once it is proved, almost invariably clears the way to the corollarial or easy theorematic proof of other propositions whose demonstrations had before been beyond the powers of the mathematicians. That is the first secondary advantage of a theoric step. The other such advantage is that when a theoric step has once been invented, it may be imitated, and its analogues applied in proving other propositions. This consideration suggests the propriety of distinguishing between varieties of theorems, although the distinctions cannot be sharply drawn. Moreover, a theorem may pass over into the class of corollaries, in consequence of an improvement in the system of logic. In that case, its new title may be appended to its old one, and it may be called a *theorem-corollary*. There are several such, pointed out by De Morgan, among the theorems of Euclid, to whom they were theorems and are reckoned as such, though to a modern exact logician they are only corollaries. If a proposition requires, indeed, for its demonstration, a theoric step, but only one of a familiar kind, that has become quite a matter of course, it may be called a *theoremation*.* If the needed theoric step is a novel one, the proposition which employs it most fully may be termed a *major theorem*; for even if it does not, as yet, appear particularly important, it is likely eventually to prove so. If the theoric invention is susceptible of wide application, it will be the basis of a mathematical method.

But mathematicians are rather seldom logicians or much interested in logic; for the two habits of mind are directly the reverse of each other; and consequently a mathematician does not care to go to the trouble, (which would often be very considerable,) of ascertaining whether the theoric step he proposes to himself to take is absolutely indispensable or not, so long as he clearly perceives that it will be exceedingly convenient; and the consequence is that many demonstrations introduce theoric steps which relieve the mind and obviate confusing complications without being logically necessary. Such demonstrations prove corollaries more easily by treating them as if they were theorems. They may be called *theoric corollaries*, or if one is not sure that they are so, *theorically proved propositions*.

* *θεωρημάτιον* is entered in L. & S., with a reference to the Diatribes of Epictetus.

I wish a historical study were made of all the remarkable theoric steps and noticeable classes of theoric steps. I do not mean a mere narrative, but a critical examination of just what and of what mode the logical efficacy of the different steps has been. Then, upon this work as a foundation, should be erected a logical classification of theoric steps; and this should be crowned with a new methodeutic of necessary reasoning. My future years,—whatever can have become of them, they do not seem so many now as they used, when, at De Morgan's *Open Sesame,* the Aladdin matmûrah of relative logic had been nearly opened to my mind's eye;—but the remains of them shall, I hope, somehow contribute toward setting such an enterprise on foot. I shall not be so short-sighted as to expect any cut-and-dried rules nor yet any higher sort of contrivance, to supersede in the least that ἀγχίνοια,—that penetrating glance at a problem that directs the mathematician to take his stand at the point from which it may be most advantageously viewed. But I do think that that faculty may be taught to nourish and strengthen itself, and to acquire a skill in fulfilling its office with less of random casting about than it as yet can.

Euclid always begins his presentation of a theorem by a statement of it in *general terms,* which is the form of statement most convenient for applying it. This was called the πρότασις, or *proposition.* To this he invariably appends, by a λέγω, "I say," a translation of it into *singular terms,* each general subject being replaced by a Greek letter that serves as the proper name for a single one of the objects denoted by that general subject. Yet the generality of the statement is not lost nor reduced, since the understanding is that the letter may be regarded as the name of any one of those objects that the student may select. This second statement was called the ἔκθεσις, or *exposition.* Euclid lived at a time when the surpassing importance of Aristotle's *Analytics* was not appreciated. The use, probably by Euclid himself, of the term πρότασις, which in Aristotle's writings means a premiss, to denote the conclusion to be proved, illustrates this, and confirms other reasons for thinking that Euclid was unacquainted with the doctrine of the *Analytics.* The invariable appending by Euclid of an ἔκθεσις to the πρότασις (except in a few cases in which the proposition is expressed in the ecthetic form alone,) inclines me to think that it was, for him, a principle of logic that any general proposition can be so stated; and such a form of statement was always convenient in demonstration; sometimes, necessary. If this surmise be correct, Euclid

probably looked upon the function of the ἔκθεσις as that of merely supplying a more convenient form for expressing no more than the πρότασις had already asserted. Yet inasmuch as the πρότασις does not mention those proper names consisting of single letters, the ἔκθεσις certainly does supply ideas that, however obvious they be, are not contained in the πρότασις; so that it must be regarded as taking a little theoric step. The principal theoric step of the demonstration is, however, taken in what immediately follows; namely, in "preparation" for the demonstration, the παρασκευή, usually translated "the construction." The Greek word is applied to any thing got up with some elaboration with a view to its being used in any contemplated undertaking: a near equivalent to a frequent use of it is " apparatus." Euclid's παρασκευή consists of precise directions for drawing certain lines, rarely for spreading out surfaces; for though his work entitled *"Elements,"* appears to have been intended as an introduction to theoretical mathematics in general, (the art of computation being the *métier,* —the 'mister, as Chaucer would say, of the Pythagoreans,) yet Euclid always conceives arithmetical quantities,—even when distinguishing between prime and composite integers,—as being lengths of lines. It was his mania. Those lines which are drawn in the παρασκευή are not only all that are referred to in the condition of the proposition, but also all the additional lines which he is about to consider in order to facilitate the demonstration of which this παρασκευή is thus the soul, since in it the principal theoric step is taken. But the construction of these additional lines is introduced by γάρ, here meaning "for," and sometimes the text does not very sharply separate some parts of the παρασκευή from the next step, the ἀπόδειξις, or demonstration. This latter contains mere corollarial reasoning, though, in consequence of its silently assuming the truth of all that has been previously proved or postulated (which Mr. Gow, in his *Short History of Greek Mathematics,* gives as the reason for Euclid's having called his work Στοίχεια; which seems to me very dubious,) this corollarial reasoning will sometimes be a little puzzling to a student who has not so thoroughly assimilated what went before as to have the approximate proposition ready to his mind. After this, a sentence always using ἄρα, "hence," *"ergo,"* repeats the πρότασις (not often the ἔκθεσις,) so as to impress the proposition on the mind of the student, in its new light and new authority, expressed in the form most convenient in future applications of it. This is called συμπέρασμα, the "conclusion," which sounds highly Aristotelian. Yet the classical use of

the verb to signify coming to a final conclusion, rendered this noun inevitable as soon as these neuter abstracts came into the frequent use that they had by Euclid's time. The conclusion always ends with the words ὅπερ ἔδει δεῖξαι, "which had to be shown," *quod erat demonstrandum,* for which Q. E. D. is now put.

I will take at random the 20th proposition of the first book, to illustrate the matter. "In every triangle, any two sides, taken together are always greater than the third.

"For let ΑΒΓ be a triangle. I say that any two sides taken together are greater than the third; ΒΑ and ΑΓ than ΒΓ, ΑΒ and ΒΓ than ΑΓ, and ΒΓ and ΓΑ than ΑΒ.

"For extend ΒΑ to the point Δ, taking ΑΔ equal to ΓΑ, [which he has shown in the 2d proposition always to be possible;] and join Δ to Γ by a straight line.

"Now since ΔΑ is equal to ΑΓ, the angle under ΑΔΓ is equal to that under ΑΓΔ [by the *pons asinorum,*] Hence, the angle under ΒΓΔ will be greater than that under ΑΔΓ. [This is a fallacy of a kind to which Euclid is subject from assuming that every figure drawn according to the παρασκευή will necessarily have its parts related in the same way, when it can only be otherwise if space is finite, which he has never formally adopted as a postulate. In the present case, if ΑΔ is more than half-way round space, the triangle ΑΓΔ will include the triangle ΑΒΓ within it; and then the angle ΒΓΔ will be less than the angle ΑΔΓ.] And since ΔΓΒ is a triangle having the angle under ΒΓΔ greater than that under ΒΔΓ, but the greater side subtends under the greater angle [which is the theorem that had just previously been demonstrated,] therefore ΔΒ is greater than ΒΓ. But ΔΑ is equal to ΑΓ. Therefore, ΒΔ and ΑΓ are greater than ΒΓ. Similarly, we shall [i. e. could] show that ΑΒ and ΒΓ are greater than ΓΑ, and ΒΓ and ΓΑ than ΑΒ.

"In every triangle, then, any two sides joined together are greater than the third, which is what had to be shown."

I will now return to the consideration of cyclical systems, and will begin by expressing my definition of such a system in those Existential Graphs which have been explained in *The Monist* (Vol. XVI, pp. 524–544, where correct the errata given in Vol. XVII, p. 160). In reference to those graphs, it is to be borne in mind that they have not been contrived with a view to being used as a calculus, but on the contrary for a purpose opposed to that. Nevertheless, if any one cares to amuse himself by drawing inferences by machinery, the graphs can be put to this work, and will perform

it with a facility about equal to that of my universal algebra of logic and as much beyond that of my algebra of dyadic relatives, of which the lamented Schroeder was so much enamoured. The only other contrivances for the purpose appear to me to be of inferior value, unless it be considered worth while to bring a pasigraphy into use. Such ridiculously exaggerated claims have been made for Peano's system, though not, so far as I am aware, by its author, that I shall prefer to refrain from expressing my opinion of its value. I will only say that if a person chooses to use the graphs to work out difficult inferences with expedition, he must devote some hours daily for a week or two to practice with it; and the most efficacious, instructive, and entertaining practice possible will be gained in working out his own method of using the graphs for his purpose. I will just give these little hints. Some slight shading with a blue pencil of the oddly enclosed areas will conduce to clearness. Abbreviate the parts of the graph that do not concern

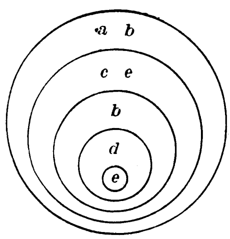

Fig. 7.

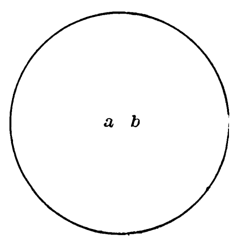

Fig. 8.

your work. Extend the rule of iteration and deiteration, by means of a few theorems which you will readily discover. Do not forget that useful iteration is almost always into an evenly enclosed area, while useful deiteration is, as usually, from an oddly enclosed area. Perform the iteration and the immediately following deiteration at one stroke, in your mind's eye. Do not forget that the ligatures may be considered as graph-instances scribed in the areas where their least enclosed parts lie, and repeated at their attachments. Their intermediate parts may be disregarded. Reflect well on each of the four permissions (especially that curious fourth one,) until you vividly comprehend the why and wherefore of each, and the bearings of each from every point of view that is habitual with you. Do not forget that an enclosure upon whose area there is a vacant cut can everywhere be inserted and erased, while an unenclosed vacant cut declares your initial assumption, first scribed, to have been absurd. You will thus, for example, be enabled to see at a

glance that from Fig. 7 can be inferred Fig. 8. The cuts perform two functions; that of denial and that of determining the order of selection of the individual objects denoted by the ligatures. If the outer cuts of any graph form a nest with no spot except in its innermost area, then all that part of the assertion that is therein expressed will need no nest of cuts, but only cuts outside of one another, none of them containing a cut with more than a single spot on it. It will seldom be advisable to apply this to a complicated case, owing to the great number of cuts required; but you should discover and stow away in some sentry-box of your mind whence the beck of any occasion may instantly summon it, the simple rule that expresses all possible complications of this principle. As an example of one of the simplest cases, Fig. 9 and Fig. 10 are seen precisely equivalent.

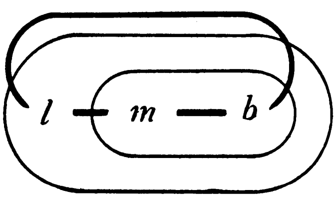

Fig. 9.

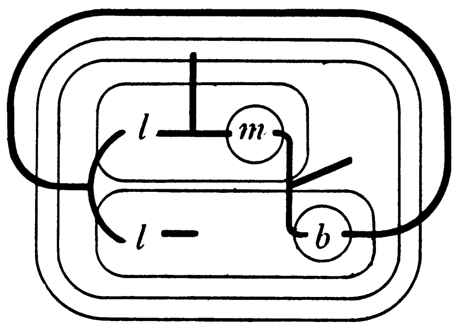

Fig. 10.

Owing to my Existential Graphs having been invented in January of 1897 and not published until October, 1906, it slipped my mind to remark when I finally did print a description of it, what any reader of the volume entitled *Studies in Logic by Members of the Johns Hopkins University*, (Boston: 1882,) might perceive, that in constructing it, I profited by entirely original ideas both of Mrs. and Mr. Fabian Franklin, as well as by a careful study of the remarkable work of O. E. Mitchell, whose early demise the world of exact logic has reason deeply to deplore.

My reason for expressing the definition of a cyclic system in Existential Graphs is that if one learns to think of relations in the forms of those graphs, one gets the most distinct and ecthetically as well as otherwise intellectually, iconic conception of them likely to suggest circumstances of theoric utility, that one can obtain in any way. The aid that the system of graphs thus affords to the process

of logical analysis, by virtue of its own analytical purity, is surprisingly great, and reaches further than one would dream. Taught to boys and girls before grammar, to the point of thorough familiarization, it would aid them through all their lives. For there are few important questions that the analysis of ideas does not help to answer. The theoretical value of the graphs, too, depends on this.

Strictly speaking, the term 'definition' has two senses,—Firstly, this term is sometimes quite accurately applied to the composite of characters which are requisite and sufficient to express the signification of the 'definitum,' or predicate defined; but I will distinguish the definition in this sense by calling it the 'definition-term.' Secondly, the word definition is correctly applied to the double assertion that the definition-term's being true of any conceivable object would always be both requisite and sufficient to justify predicating the definitum of that object. I will distinguish the definition in this sense by calling it the 'definition-assertion-pair.' In the present case, as in most cases, it is needless and would be inconvenient to express the entire definition-assertion-pair with strict accuracy, since we only want the definition in order to prove certain existential facts of subjects of which we *assume* that the definitum, 'cyclic-system,' is predicable. We do not care to *prove* that it is predicable, and therefore the assertion that the definitum is predicable of the definition-term is not relevant to our purpose. In the second place, we do not care to meddle with that universe of concepts with which the definition deals; and it would considerably complicate our premisses to no purpose to introduce it. We only care for the predication of the definition-term concerning the definitum so far as it can concern existential facts. All that we care to express in our graph is so much as may be required to deduce every existential fact implied in the existence of a cyclic system.

A cyclic system is a system; and a system is a collection having a regular relation between its members. One member suffices to make a collection, and is requisite to the existence of the collection. The definition, so far as we need it, is then expressed in the graph of Fig. 11. Here K with a "peg" (See *Monist,* Vol. XVI, p. 530) at the side asserts that the object denoted by the peg is a cyclic system. The letter M with one peg at the top and another placed on either side without any distinction of meaning, asserts that the object denoted by the side-peg is a *member* of the system denoted by the top-peg. The letter C, with a peg at the top and another at the side asserts that the object denoted by the top-peg is a relation

involved in that relation between all the members which constitutes the entire collection of them as the system that it is, and asserts that the object denoted by the side-peg is such a system. The Roman numerals each having one peg placed at the top or bottom of the numeral and a number of side-pegs equal to the value of the numeral, all these side-pegs being carefully distinguished, is used to express the truth of the proposition resulting from filling the blanks of the rheme denoted by the top or bottom peg, with indefinite signs of objects denoted by the side-pegs taken in their order, all the left-hand pegs being understood to precede all the right-hand pegs, and on each side a higher peg to precede a lower one. With this understanding, the graph of Fig. 11, where for the sake of perspicuity the oddly enclosed, or negating areas are shaded, may be translated into the language of speech in either of the two following equivalent forms (besides many others):

It is false that
  there is a cyclic system while it is false that
    this system has a member
    and involves a relation ("being A to," the bottom peg of II,)
    and that it is false that
      the system has a member of which it is false that
        it is in that relation, A, to a member of the system,
      while it is false that
        there is a definite predicate, P, (the top or bottom peg of I,) that is true of a member of
        the system and is false of a member of the system, and that it is false that
          this predicate is true of a member of the system of which it is false that
            it is A to a member of the system of which P is true.

This more analytic statement is equivalent to saying that every cyclic system (if there be any,) has a member, and involves a relation called "being A to" (not the graph but perspicuity of speech requires it to be so named,) such that every member of the system is A to a member of the system, and any definite predicate, P, whatsoever, that is at once true of one member of the system and untrue of another is true of some member of the system that is not A to any member of which P is true.

To anybody who has no notion of logic this may seem a queer attempt to explain what is meant by a cyclic system; and it is true that it would be a needlessly involved *verbal* definition; a verbal

definition being an explanation of the meaning of a word or phrase intended for a person to whose mind the idea expressed is perfectly distinct. But it is not intended to serve as a *verbal,* but as a *real* definition, that is, to explain to a person to whom the idea may be familiar enough, but who has never picked it to pieces and marked its structure, exactly how the idea is composed. As such, I believe it to be the simplest and most straight-forward explanation possible. When you say that the days of the week "come round in a set of seven," you think of the week everything here expressed of K. I do not mean that all this is *actually* existent in your thought; for thinking no more needs the actual presence in the mind of what is

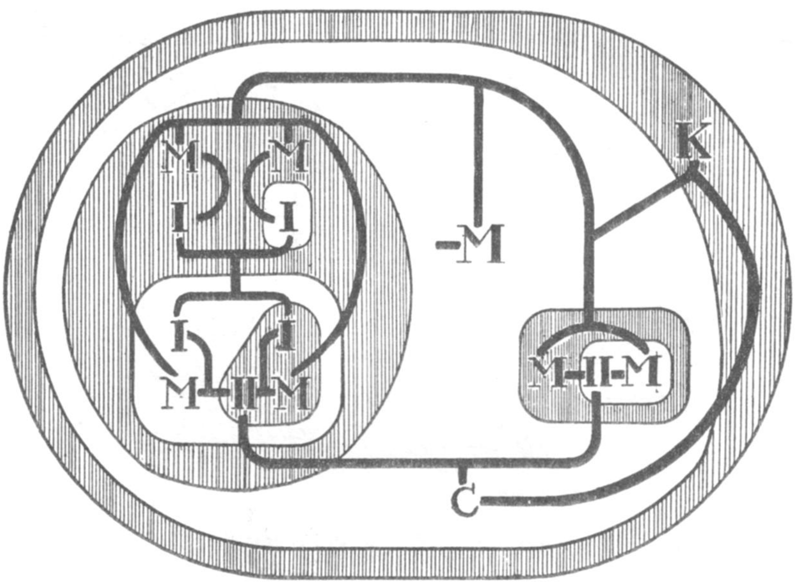

Fig. 11.

thought than knowing the English language means that at every instant while one knows it the whole dictionary is actually present to his mind. Indeed, thinking, if possible, even *less* implies presence to the mind than knowing does; for it is tolerably certain that a mind to whom a word is present with a sense of familiarity knows that word; whereas a mind which being asked to *think* of anything, say a locomotive, simply calls up an image of a locomotive has, in all probability, by bad training, pretty nearly lost the power of thinking; for really to think of the locomotive means to put oneself in readiness to attach to it any of its essential characters that there

may be occasion to consider; and this must be done by general signs, not by an image of the object. But the truth of the matter will more fully be brought out as we proceed.

All that we require of the definition may be put into a simpler shape by omitting the letter M, since the interpreter of the graph must well understand that the whole talk of the graphist for the time being, so far as it refers to things and not to the attributes or relations, has reference to the members of a cyclic system. We may consequently use the graph of Fig. 12 in place of Fig. 11.

It will be remarked that the graph of Fig. 12 is no more a definition of a cyclic system than it is of the relation of immediate antecedence; and this is as it should be; for plainly a system cannot

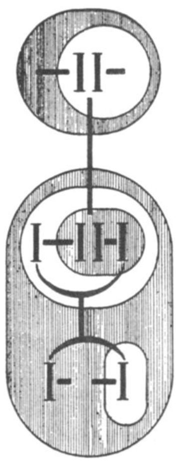

Fig. 12

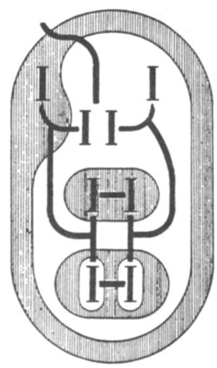

Fig. 13.

be defined, without virtually defining the relation between its members that constitutes it a system.

I will now begin by drawing one of several corollaries that are right at my hand. I am always using the words '*corollary*' and *theorem* in the strict sense of the foregoing definition. This corollary results from the logical principle that to every predicate there is a negative predicate which is true if the former is false, and is false if the former is true. This purely logical principle is expressed in the graph of Fig. 13. Obviously, if any predicate is both true of some member and false of some member of the system, the same will be the case with its negative. Consequently, by the definition, this negative will be true of some member without being true of any to which that member is A; or, in other words, the original

predicate will be false of some member without being A to any member of which it is false. Thus, if any predicate is neither true of all nor false of all the members of any cyclic system, but is true of some one and false of some other, there will be two different members of one of which it is true without being true of any to which that member is A, while of the other it is false without being false of any to which that member is A. Or, to put the corollary in a different light, taking any predicate, P, whatsoever, then, in case you can prove that there cannot be more than one exception to the rule that every member of the system resembles some one of those to which it is A in respect to the truth or falsity concerning it of P, then if P be true of one member, it is true of all, and if it be false of one, it is false of all.

I am now going to apply this proposition to a theoric proof of a proposition which is really only a corollary from the definition of a cyclic system. My motive for this departure from good method is that it will afford a good illustration of the advantage of making the selected predicate, P, as special and characteristic of the state of things you are reasoning about as possible. The proposition I am going to prove is, that in any cyclic system that contains more than one member no member will be A to itself. For this purpose I will consider any member of the system you please, and will give it the proper name, N. This ecthetic step is already theoric, but is a matter of course. Another theoric step, not a matter of course, shall consist in my selecting, as the predicate to be considered, "is N." Now if N is A to itself, every member of the system of which this predicate is true (which can be none other than N itself,) will be A to a member of which the predicate is also true; and consequently, by the definition of a cyclic system this predicate cannot be true of one member and false of another. But if there be any other member of the system than N, it will be false of that one. Whence, if N were A to itself and were not the only member of the system, there would be no member of which it would be true that it was N. But by the definition, every cyclic system has some member, and N was chosen as such. So that it must be, either that the system has no other member, or that any member you please, and consequently *every one,* is non-A to itself.

Now what I wanted to point out was that if instead of "is N," I had selected, as my predicate to be considered, "is A to itself," it would merely have followed that since any member that is A to

itself is A to a member that is A to itself, by the general definition either every member of the system is A to itself or none is so.

I will now prove that this proposition that no member of a cyclical system is A to itself unless it is the only member of the system is not a theorem, in any strict sense, by proving it corollarially. For this purpose I first prove that no cyclical system, by virtue of the same relation A, involves another as a part, but not the whole of it. For suppose that certain members of a cyclical system form by themselves a cyclical system constituted by the same A-hood. Then, by the part of the definition of a cyclical system that has been expressed as graph in Fig. 11 and in Fig. 12, there is a member of this minor system; and every member of it is A to a member of the major system that is a member of the minor system. Hence, by that same partial definition, the predicate "is a member of the minor system" being true of one is true of all members of the major system. The minor system is, then, the whole of the major system. To go further, I must employ that assertion of the definitum "is a cyclic system" concerning the definition-term, which assertion has not been expressed as a graph, in order to prove, by its conformity with the definition that a single object, having a relation, identity, to itself, that relation conforming to the conditions of the constitutive relation of a cyclical system, must be admitted to be a cyclical system of a single member. If, therefore, one of the members of a cyclical system of more than one member were A to itself, it would be a cyclical system which was a part but not the whole of another cyclical system, which we have seen to be impossible.

I shall now employ the first corollary to prove that every member of a cyclical system is A'd by some member. For take any member you please of any such system you please; and I will assign to it the proper name N. If then N is the only member of the system, by the definition N is A to itself. But if there be another member, it is one of which the predicate "is N" is not true, though there is some member, namely N, of which that predicate is true. Consequently, by that first corollary, there must be a member of which it is not true that it is N which is A to nothing of which this is not true. But, by the definition, every member of a cyclic system is A to some member; and therefore that member which is not A to any member of which "is N" is not true, must be true of a member of which "is N" is true, which, by hypothesis, is only N itself; consequently any member of any cyclic system which one may choose

to select is A'd by some member, and by another than itself, if there be another. Q. E. D.

Further investigation of the properties of cyclic systems will need a somewhat more recondite theoric step. Certainly, however, I must not convey the idea that I claim to be quite sure of this. As yet, I have not sufficiently studied the methodeutic of theorematic reasoning. I only have an indistinct apprehension of a principle which seems to me to prove what I say; and I must confess that of all logical habits that of confiding in deductions from vague conceptions is quite the most vicious, since it is just such reasonings that to the intellectual rabble are the most convincing; so that the conclusions get woven into the general common-sense so closely, that it at length seems paradoxical and absurd to deny them, and men of "good sense" cling to them long after they have been clearly disproved. However, whether it be absolutely necessary or not, the only way I see, at present, of demonstrating the remaining properties of a cyclic system is to suppose a predicate to be formed by a process which will seem somewhat complicated. I shall not state what this predicate is, but only suppose it to be formed according to a rule; and even this rule will not be exactly stated but only a description of its provisions will be given. I shall suppose that one member of the system is selected by the rule as one of the class of subjects of which the predicate is true, and that the remaining members of this class shall be taken into it from among the members of the system *one by one,* according to the rule that when the member last taken in is not A to any member already taken in, one and one only of the members of the system not yet taken in to which that last adopted member is A is to be added to the class; and this new addition may, in the same way, require another. If the system were infinite (as we shall soon see that it cannot be,) this might go on endlessly; and so far, we have not seen that this cannot happen. But as soon as it happens that the member last admitted to the class is A to a member already admitted (and consequently that every member admitted to the class is A to an admitted member) the admissions to the class are to be brought to a stop. There are now two supposable cases to be provided for which we shall later find will never occur; but if we did not determine what was to be done if they should (this not being proved impossible) our first proof would involve a *petitio principii.* One is the case in which the finally adopted member is A to a member already having an A that had previously been admitted to the class. The other is

the case in which the last (but not necessarily the final) adopted member is not only A'd by the *last previously* adopted member (for the sake of providing which with a member A'd by it, the very last was taken in) but is also A'd by an earlier adopted member. In the latter case, in which the member last adopted, which we may name V, is not only A'd by the last previous one, which we may name U, but is also A'd by a previously adopted member of the class which we may name K, we are to reject from the class all that were admitted after K to U inclusive; so that we revert to what would have been the case, as it might have been, if next after K we had admitted V, to which K is A. We should thus make the class smaller, which we shall soon see could not happen. In the other case, where the last adopted member, which we will name, Z, is A to a previously adopted one, which we will name J, which was not the first member adopted into the class, but is A'd by another, which we will name I, we reject from the class both I and all that were adopted previously to I.

After these supposititious rejections, there is no object of which the predicate, "is a member of the class so formed," is true that is not A of any object of which the same predicate is true, and therefore, by the definition so often appealed to, this predicate cannot be both true of a member of the cyclic system and false of another such member. Now it plainly is true of some member, since the first object taken into it as well as every one subsequently taken into it were members of the cyclic system. Therefore, this predicate cannot be false of any member of the cyclic system. In other words, the class so formed includes all the members of the cyclic system. Consequently, there cannot have been any rejections.

Since there were no rejections, the first member adopted must remain a member of the class; and since we have seen in a former corollary that every member of a cyclic system is A'd by a member of the same system, this first adopted member must be A'd by some member of the system, that is, by some member of the class. But by the rule of formation of the class no member of it except the finally adopted one can be A to a previously adopted member. It follows that there must be a finally adopted one; and by the same rule no member of the class except the first was adopted without there being a *last previously adopted* member. It follows that the succession of adoptions cannot, at any part of it, have been endless. This is one of the most difficult theorems that I had to prove.

Moreover, every member of the class is by the mode of forma-

tion A to one, and only to one, member of the class: and consequently the same is true of all the members of every cyclic system.

Moreover, every member of the class except the first was only taken in so as to be A'd by the last, or, at any rate, by one member only; and the first adopted member as we have seen is A'd by the finally adopted member. It cannot be A'd by any other, since by the rule of formation, such another would thereby have become the finally adopted member. Hence, no member of a cyclic system is A'd (in the same sense) by any two members of the system; or no two members are A to the same member.

I have thus, by means of this θεωρία of the formation of a certain kind of class, succeeded in demonstrating, what one might well have doubted, that from the proposition expressed in Fig. 11 follows the double uniqueness of the cyclical relation of A-hood or immediate antecedence. This is the principal, as I think, of those properties that are common and peculiar to cyclical systems. The same theoric step, or a reduplication of it, will enable the reader to prove other properties, common but not peculiar to cyclic systems; and especially that a collection the count of whose members in one order comes to an end can never in any order involve an endless process, whether it comes to an end or does not. There is, by the way, an important logical interest in that mode of succession in which an endless succession, say, of odd numbers, is followed by a beginningless diminishing succession of even numbers. For it shows that two classes of objects may have such a connection with a transitive relation, such as are those of causation, logical implication, etc., that any member of either class is *immediately* in this relation only to a member of the same class, while yet every member of one of the classes may be in this same relation to every member of the other class. Thus, it may be that thought only acts upon thought *immediately*, and matter *immediately* only upon matter; and yet it may be that thought acts on matter and matter upon thought, as in fact is plainly the case, somehow.

In this theoric step, it is noticeable that I have had to embody the idea of *antecedence* generally, in order to prove the properties of cyclical *immediate antecedence*. Any reasoner is always entitled to assume that the mind to which he makes appeal is familiar with the properties of antecedence in general; since if he were not so, he could not even understand what reasoning was at all about. For logical antecedence is an idea which no reasoner can unload or dis-

pense with. It would have been easy to replace, in my demonstrations, all the "previously"s etc. by relations of inference. I have not done so in order not to burden the reader's mind with needlessly intricate forms of thought.

A corollary from what has already been proved is that if we regard the definition of Fig. 12 as the definition of A-hood, or cyclical immediate antecedence, then A-hood is not a single relation but is any one of a class of relations which, if the collection of all the members of the system is not very small, is a large class. For taking any two members of the system, and naming them Y and ϒ, we may form such a relation, that of A′-hood, that whatever is neither Y nor ϒ, nor is A to Y nor to ϒ is A′ to whatever is A'd by it, while whatever is A to Y is A′ to ϒ, whatever is A to ϒ is A′ to Y, whatever is A'd by Y is A″d by ϒ, and whatever is A'd by ϒ is A″d by Y; and then A′ will have the same general properties as A. Thus, if the number of members of a cyclic system is *m*, the number of relations of A-hood is $(m-1)!$ If *m* be seven, the number of A-relations is 720; etc.

There is no relation in a cyclic system exactly answering to general antecedence in a denumeral* system.

As a finitude is a positive complication (as is shown by a form of inference being valid in a finite system that is not elsewhere valid,) so in place of the relation of betweenness which in a linear system endless both ways, which, if those ways are not distinctively characterized, is triadic, we have in a cyclic system a tetradic relation expressible by α with four tails, so that Fig. 14, which means that an object which can, wherever it be in the cycle, pass from its position to that which is *next* to that position, being either A to it or A'd by it, will if at I be *opposite* to an object at J, relatively to any objects at U and at V. That is, such an object cannot move from I to J without passing through U and V. This implies that U is opposite to V relatively to I and J; that no other pair out of the four are opposite to each other relatively to the other pair; and that that way of passing round the cycle in which U is reached next after I is the way in which J is reached next after U, V next after J, and I next after V; while that way in which V is reached next after I is the way in which J is reached next after V, U next after J, and I next after U. This supposes that I, J, U, and V are all different, as those that are opposite must be unless two that are

* See Note at the end of the article.

adjacent are identical, in which case we may understand the relation as always being true and meaningless.

We may modify this relation, so as to render it exact, by de-

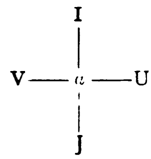

Fig. 14.

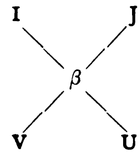

Fig. 15.

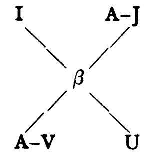

Fig. 16.

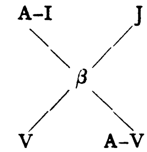

Fig. 17.

fining Fig. 15 as true, if I and J are identical while U and V are also identical; or if I and U are identical while J and V are identical, and also if Fig. 16 or Fig. 17 is true; but as not true unless necessarily so according to these principles. This last clause, by the way, has a very important logical form; but I shall not stop to comment upon it.

It will be observed that if Fig. 15 is true, then one or other of the graphs of Fig. 18 must be true. And if two α-relations hold,

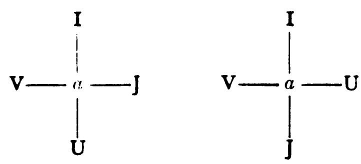

Fig. 18.

having three of their four correlates identical, and not the same pair being opposite in both, then two α-conclusions may be drawn in which the two correlates that only appeared once each in the premis-

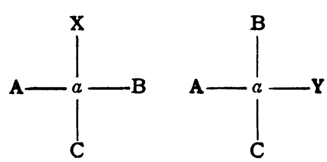

Fig. 19.

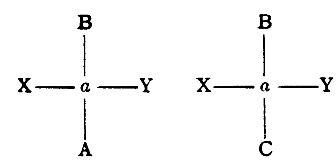

Fig. 20.

ses, appear together, and opposite to one another. Thus, from Fig. 19 may be inferred Fig. 20. The β-relation lends itself to much further inferential procedure. In the first place in Fig. 15, the whole graph may be turned round on the paper so as to bring each correlate into the place of its opposite. It may also be turned through 180° round a vertical axis in the sheet. [It may consequently be turned 180° round a horizontal axis in the sheet.] Moreover, the

two correlates on the left, I and V, may be interchanged. [And so, consequently may J and U.] Moreover, from Fig. 21, we can infer Fig. 22. [Whence it follows that from Fig. 23 we can infer Fig

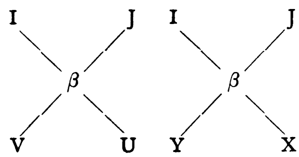

Fig. 21.

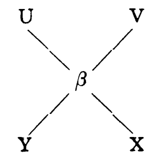

Fig. 22.

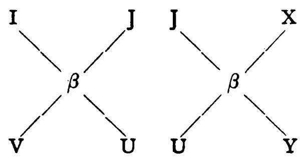

Fig. 23.

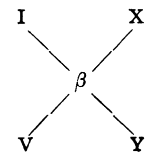

Fig. 24.

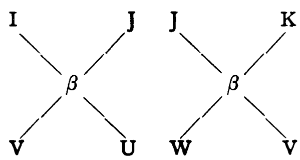

Fig. 25.

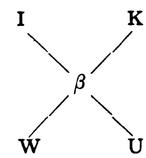

Fig. 26.

24.] Also, from Fig. 25 we can infer Fig. 26. Whence there follow very obviously several transformations. For example, Fig. 27 will be true; and if any three of the four graphs of Fig. 28 are true, so is the other one. It is obvious that the relation β involves cyclical

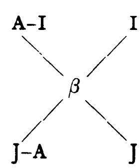

Fig. 27.

addition-subtraction, by its definition. Cyclic arithmetic involves no other *ordinal,* or climacote, numbers than cyclic ordinals. But if we define a *cardinal* number as an adjective essentially applicable, universally and exclusively, to a plural of a single multitude, then even the relations α and β may be said to depend upon the value of a cardinal number; namely, upon the modulus of the cycle; and no cardinal number is cyclic. Dedekind and others consider the

pure abstract integers to be ordinal; and in my opinion they are not only right, but might extend the assertion to all real numbers. [But what I mean by an ordinal number precisely must be explained further on.] Nevertheless, the operations of addition, mul-

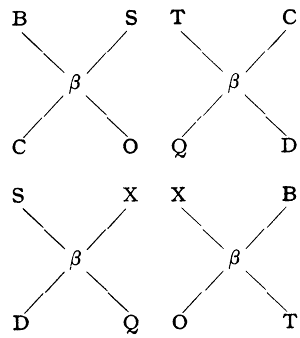

Fig. 28.

tiplication, and involution can be more simply defined if they are regarded as applied to cardinals, that is to multitudes, than if they are regarded in their application to ordinals.

Thus, the sum of two multitudes, M and N, is simply the multitude of a collection composed of the mutually exclusive collections of the multitudes M and N. The ordinal definition, on the other hand, must be that $0+X=X$, whatever X may be, while (the ordinal next after Y)+X is the ordinal next after $(Y+X)$. So the product of two multitudes M and N, is simply the multitude of units each composed of a unit of a collection of multitude M and a unit of multitude N; while the ordinal definition must be that $0\times 0=0$ and that $X\times$(the ordinal next after Y) is $X+(X\cdot Y)$ and the ordinal next after $X\times Y$ is $(X\cdot Y)+Y$. So finally the multitude M raised to the power whose exponent is N, is the multitude of ways in which every member of a collection of multitude N can be related in a given

X	Y	Z	X	Y	Z	X	Y	Z
A B			A	B		A		B
X	Y	Z	X	Y	Z	X	Y	Z
B	A			A B			A	B
X	Y	Z	X	Y	Z	X	Y	Z
B		A		B	A			A B

Fig. 29.

way, each to some single member or other of a collection of multitude M. Thus $3^2=9$ because the different configurations of Fig. 29 are

nine in number; while $2^3=8$ because the different configurations of Fig. 30 are eight in number. But a definition of involution which

A	B	A	B	A	B	A	B
X Y Z		X Y	Z	X Z	Y	X	Y Z
A	**B**	**A**	**B**	**A**	**B**	**A**	**B**
Y Z	X	Y	X Z	Z	X Y		X Y Z

Fig. 30.

shall be *purely* ordinal must be quite a complicated affair. We may say, for example, that $X^1=X$ and $X^{1+Y}=X \cdot X^Y$.

In cyclic addition, that is, in the α and β relations, there is but a single cardinal number to be dealt with; and this is fully dealt with in counting round and round the single cycle. But in multiplication there is always another cycle, and thus another cardinal number to be considered, although the modulus of the second cycle is usually such that it is not brought to our attention. But suppose that in a cycle of 72 we multiply the successive integers from zero up by 54. The following will be the result:

$$\begin{aligned} 0\times 54 &= 0 = 72\\ 1\times 54 &= 54 = -18\\ 2\times 54 &= 36\\ 3\times 54 &= 18\\ 4\times 54 &= 72 = 0 \end{aligned}$$

It will be seen that there is a cycle of modulus 4. Suppose that, instead of 54, we take 27 as the multiplicand. Then we shall have

$$\begin{aligned} 0\times 27 &= 0 = 72\\ 1\times 27 &= 27\\ 2\times 27 &= 54 = -18\\ 3\times 27 &= 9\\ 4\times 27 &= 36\\ 5\times 27 &= 63 = -9\\ 6\times 27 &= 18\\ 7\times 27 &= 45 = -27\\ 8\times 27 &= 72 = 0 \end{aligned}$$

By halving the multiplicand we have doubled the modulus. Suppose, however, that, instead of $\frac{1}{2}\times 54$, we take $\frac{1}{3}54=18$, as the multiplicand. Read the column of successive multiples of 54 upwards, and we shall see that the multiples of 18 have a cycle of modulus 4.

With 6 as the multiplicand we get a cycle of 12 for its multiples, the numbers being as follows:

6, 12, 18, 24, 30, 36, –30, –24, –18, –12, –6, 0

With 2×6 we get a cycle of $\frac{1}{2}12$, every other one. With 4×6 as multiplicand, we get a cycle of $\frac{1}{4}12=3$, with 8×12 as multiplicand, since 3 cannot be halved we still get 3. With $3\cdot 6=18$ as multiplicand, we get a cycle of $\frac{1}{3}\times 12$, or every third of the multiples of 6, but with $3\cdot 18=54$ as modulus, since 4 is not divisible by 3, we still get a cycle of 4. With $6\cdot 6=36$ as multiplicand, we get every sixth multiple of 6, or two in all, 0 and 36. With 5×6, 7×6, and 11×6 since 12 is not divisible by 5, 7, or 11, we still get a modulus of 12. With 30, the order is as follows:

$$0, 30, -12, 18, -24, 6, 36, -6, 24, -18, 12, -30, 0.$$

This principle is obvious: if the multiples of a number N form a cycle of modulus K, and p is a prime number, then the multiples of pN will form a cycle of K/p, provided K is divisible by p; but otherwise, the modulus will remain K. Suppose, then, that the cycle of multiples of 1, that is to say, the cycle of our entire system of numbers is $p^a\cdot q^b$, where p and q are primes, and a and b are any whole numbers. If, then, we multiply 1 by $r^c\cdot s^d\cdot t^e$, where r, s, t are other primes than p and q, the modulus of the cycle of multiples of $r^c\cdot s^d\cdot t^e$ will remain $p^a\cdot q^b$. But every time we multiply this by p we divide the modulus by p, until we have so multiplied it a times. On the other hand, if, instead of multiplying 1 by $r^c\cdot s^d\cdot t^e$, we multiply it by $p^a\cdot q^b$ to get a new multiplicand, the modulus of the cycle of multiples of $p^a\cdot q^b$ will be 1; that is, all multiples will be equal. It will follow by the distributive principle, that $p^a\cdot q^b$ added to any number leaves that number unchanged. That is to say, the modulus of a cycle is the *zero* of that cycle. But right here I must explain what I mean by an *ordinal number*.

Take any enumerable, or finite, collection of distinct objects. Let there be recognized one special relation in which each of them stands to a single one of them, and no two to the same one, and such that any predicate whatsoever that is true of any one of them and is true of the one to which any one of which it is true stands in that relation, is true of all of them. This substantially defines that relation as the relation of "being A'd by." Thereby, that collection is recognized as forming a cyclical system of which those objects are members. But those objects will not in general be numbers of any kind. They may be days of the week or certain meridians of the Globe. But now consider a single "step," or substitution, by which the A of any member of the cyclic system is replaced by the member itself. From what member this step, or substitution began remains indefinite. The "step" still leads to a single

member, and the step is a single kind of step even if that member be any member you please, in which case it is not a single, i. e. a singular, but the general member. I will condescend to meet the reader's probably indurated habit of crass nominalist thought by saying that, in the one case, it is a single member not definitely described, and in the other is a single member, left to him to choose; and there is no objection to this, if the member be supposed to be both existent and intelligible, both of which however it need not be. Give this kind of a step a proper name. Next consider in succession all the kinds of step each of which consists in first taking a step of the last previously considered kind and then substituting for the member which it puts in place of another, the member of which that member is A; so that the kinds of steps may be

From the A of a member to that member,
From the A of the A of a member to that member,
From the A of the A of the A of a member to that member,
etc. etc.

Now if each of these has a name, whether pronounced, scribed, or merely thought, those names will come round in a cycle of the same modulus as the original system. They will therefore form a cyclic system, but not a system of objects not essentially ordered, as the original system may have been. This system of names is a cyclic system of numbers. These are ordinal, or climacote, numbers. By ordinal numbers in general I mean names essentially denoting kinds of steps each from any member whatever of a system of objects to, at most, a single object of the system, (i. e., one or another object, depending on what object the step replaces by this other). Thus, as I use the term "ordinal number" I do not mean the absolute first, second, third, etc. member of a row of objects, but rather such as these: the same as, the first after, the second after, the third before, etc. These numbers are certainly "ordinal" in the sense of expressing relative order; yet it might be better to avoid possible misunderstanding by calling them *metrical numbers,* or more specifically, *climacode* or *climacote numbers.*

In order to push further our study of this subject, let us suppose a pack of 72 cards, numbered in order upon their faces, to be dealt into two piles. We will not directly consider those serial face-values, but only their differences. The two piles cannot regularly be reunited, because the difference of successive face-values in each, comes round in a cycle in each pile, the bottom card of the one pile, 1, being 2 more than the top card 71 (counting round the cycle of

modulus 72) and that of the other pile also coming round in a cycle. The difference between the face-values of any two cards in either pile is a multiple of 2, the multiplier being the difference of position in that pile. If now we desire so to re-deal the cards of the one pile and the other into any number n of piles, as to produce the same effect as if they had originally been dealt into $2n$ piles, we must first deal the first pile leaving room between every two of the new piles for the piles to be produced by dealing the second pile. If for the number, n, we take 8, we shall get sixteen piles, the first 8 of 5 cards each and the last 5 of 4; and now it is allowable and proper to place each of the first 8 piles on the pile 8 piles further advanced; or equally so to place each of the last 8 piles on the pile 8 piles further advanced, counting round and round the cycle of modulus 16. In either case the cards of each composite pile so formed will form a cycle, successive face-values increasing (round and round the cycle of 72) by 16. The rule for gathering the piles is just the same as that previously given, except that one must confine oneself to piles *of the same set.* For instance if 72 cards, numbered as just described, get in any way dealt into 15 piles, the top cards of the piles will have these values:

61, 62, 63, 64, 65, 66, 67, 68, 69, 70, 71, 72, 58, 59, 60

Now since 15|72=3 these are in 5 sets of 3 piles, thus

61, 64, 67, 70, 58,
62, 65, 68, 71, 59,
63, 66, 69, 72, 60.

We shall therefore put the pile headed by 72 on the pile headed by 69, because there is only one pile of the set to the right of the former, and these on the pile headed by 66, and these on that headed by 63, and finally all four on the one headed by 60. So we shall in the next set begin with the pile headed by 71, the last of the larger piles.

We shall thus get the whole pack divided into three portions, and there is absolutely no way of getting them back into a single pack except by *undealing* them, that is by cutting the cards one by one from the three portions in turn, round and round.

This general rule holds in all cases; as much when the entire number of cards is prime as when it is composite. For a prime number is one whose greatest common divisor with any smaller positive integer is 1, while, of course, like any other number, its greatest divisor common to itself is itself.

Having thus fully explained the dealing into any number of

piles of any number of cards, prime or composite, I revert, after this almost interminable disquisition, to the subject of cyclic logarithms. I have confined, and shall continue to confine, my study of these to logarithms of numbers whose cycle has a prime modulus. Then, the modulus of the cycle of the logarithms being one less than that of the natural numbers cannot be prime. Still so long as it is a question of employing the logarithms merely to multiply two numbers, the logarithm of the product is simply the sum of the logarithms of multiplier and multiplicand; and in addition it makes no difference whether the modulus be prime or composite. But when it comes to raising numbers to powers or to extracting their roots, the divisors of the number one less than the modulus have to be considered. The modulus being prime, the number one less must be divisible by 2. If 2 be the only prime factor, the modulus must be 3 or 5 or 17 or 65537 or much greater yet. As an example, let us take the modulus 17. Then the following two pairs of tables show the logarithms for the 8 different bases 3, 5, 6, 7, 10, 11, 12, 14.

Nat. nos.	-16	-14	-8	-7	-4	-12	-2	-6	-1	-3	-9	-10	-13	-5	-15	-11	-16
	1	3	9	10	13	5	15	11	16	14	8	7	4	12	2	6	1
Logs.	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
	-16	-15	-14	-13	-12	-11	-10	-9	-8	-7	-6	-5	-4	-3	-2	-1	0

Nat. nos.	-16	-15	-14	-13	-12	-11	-10	-9	-8	-7	-6	-5	-4	-3	-2	-1
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Logs.	0	14	1	12	5	15	11	10	2	3	7	13	4	9	6	8
	-16	-2	-15	-4	-11	-1	-5	-6	-14	-13	-9	-3	-12	-7	-10	-8

Nat. nos.	-16	-12	-9	-11	-4	-3	-15	-7	-1	-5	-8	-6	-13	-14	-2	-10
	1	5	8	6	13	14	2	10	16	12	9	11	4	3	15	7
Logs.	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
	-16	-15	-14	-13	-12	-11	-10	-9	-8	-7	-6	-5	-4	-3	-2	-1

Nat. nos.	-16	-15	-14	-13	-12	-11	-10	-9	-8	-7	-6	-5	-4	-3	-2	-1
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Logs.	0	6	13	12	1	3	15	2	10	7	11	9	4	5	14	8
	-16	-10	-3	-4	-15	-13	-1	-14	-6	-9	-5	-7	-12	-11	-2	-8

Of course, none of the even numbers can be logarithms of a possible base of another system since with a modulus 16 no multiple of an even number can be 1, the logarithm of the base. On the other hand, every odd number is in every system of logarithms the logarithm of some base.

If, instead of 13 cards and 12, the "trick" be done with 17 and

16, say the first eight hearts *increasingly* and then the first eight diamonds *decreasingly,* with the joker or king of hearts to make up 17 and with the first eight spades to correspond with the hearts and the first eight clubs to correspond with the diamonds, laying down the black cards on the table, in *two* rows, one of eight from left to right, and the other below from right to left, after having dealt the black cards 16 times into three piles and every time exchanging the top card of the middle pile for the topmost red card, so as to bring the ace of spades into the right-hand-most place of the upper row, then having done the trick substantially as above described, there is a very pretty way in which you can ask into what *odd* number of piles the *black* cards shall be dealt and then dealing out the red cards, *minus* the extra one 16 times exchanging a card each time for the three court cards and ten of each suit, so as to again render the black ones the index of the places of the red ones. But I leave it to the reader's ingenuity to find out exactly how this is to be done. *Beware of the moduli.*

There is much more to be said on this subject, but I leave it for the reader to investigate.

CHARLES SANTIAGO SANDERS PEIRCE.

MILFORD, PA.

NOTE REFERRED TO ON PAGE 452.

Denumeral is applied to a collection in one-to-one correspondence to a collection in which every member is immediately followed by a single other member, and in which but a single member does not, immediately or mediately, follow any other. A collection is in one-to-one correspondence to another, if, and only if, there is a relation, r, such that every member of the first collection is r to some member of the second to which no other member of the first is r, while to every member of the second some member of the first is r, without being r to any other member of the second. The positive integers form the most obviously denumeral system. So does the system of all real integers, which, by the way; does not pass through infinity, since infinity itself is not part of the system. So does a Cantorian collection in which the endless series of all positive integers is immediately followed by ω_1, and this by ω_1+1, this by ω_1+2, and so on endlessly, this endless series being immediately followed by $2\omega_1$. Upon this follow an endless series of endless series all positive integer coefficients of ω_1 being exhausted, whereupon immediately follows ω_1^2, and in due course $x\omega_1^2+y\omega_1+z$, where x, y, z, are integers; and so on; in short, any system in which every member can be described so as to distinguish it from every other by a finite number of characters joined together in a finite number of ways, is a denumeral system. For writing the positive whole numbers in any way, most systematically thus:

1, 10, 11, 100, 101, 110, 111, 1000, 1001, 1010, 1011, etc.

it is plain that an infinite square matrix of pairs of such numbers can be arranged in one series, by proceeding along successive bevel lines thus: (1, 1); (1, 10); (10, 1): (1, 11); (10, 10); (11, 1): (1, 100); (10; 11); (11; 10); etc. and consequently whatever can be arranged in such a square can be arranged in one row.

Thus an endless square of quaternions such as the following can be so arranged:

[(1,1) (1,1)] : [(1,1) (1,10)]; [(1,1) (10,1)] : [(1,1) (1,11)]; etc.
[(1,10) (1,1)] : [(1,10) (1,10)]; [(1,10) (10,1)] : [(1,10) (1,11)]; etc.
[(10,1) (1,1)] : [(10,1) (1,10)]; [(10,1) (10,1)] : [(10,1) (1,11)]; etc.
[(1,11) (1,1)] : [(1,11) (1,10)]; [(1,11) (10,1)] : [(1,11) (1,11)]; etc.

Consequently whatever can be arranged in a block of any finite number of dimensions can be arranged in a linear succession. Thus it becomes evident that any collection of objects, every one of which can be distinguished from all others by a finite collection of marks joined in a finite number of ways can be of no greater than the denumeral multitude. (The bearing of this upon Cantor's ω^{ω} is not very clear to my mind.) But when we come to the collection of all irrational fractions, to exactly distinguish each of which from all others would require an endless series of decimal places, we reach a greater multitude, or grade of maniness, namely, the *first abnumerable multitude*. It is called "abnumerable," to mean that there is, not only no way of counting the single members of such a collection so that, at last, every one will have been counted (in which case the multitude would be *enumerable*), but, further, there is no way of counting them so that every member will after a while get counted (which is the case with the single multitude called *denumeral*). It is called the *first* abnumerable multitude, because it is the smallest of an endless succession of abnumerable multitudes each smaller than the next. For whatever multitude of a collection of single members μ may denote, 2^{μ}, or the multitude of different collections, in such collection of multitude μ, is always greater than μ. The different members of an abnumerable collection are not capable of being distinguished, each one from all others, by any finite collection of marks or of finite sets of marks. But by the very definition of the first abnumerable multitude, as being the multitude of collections (or we might as well say of denumeral collections) that exist among the members of a denumeral collection, it follows that all the members of a first-abnumerable collection are capable of being ranged in a linear series, and of being so described that, of any two, we can tell which comes earlier in the series. For the two denumeral collections being each serially arranged, so that there is in each a first member and a singular next later member after each member, there will be a definite first member in respect to containing or not containing which the two collections differ, and we may adopt either the rule that the collection that contains, or the rule that the collection that does not contain, this member shall be earlier in the series of collections. Consequently a first-abnumerable collection is capable of having all its members arranged in a linear series. But if we define a *pure* abnumerable collection as a collection of all collections of members of a denumeral collection each of which includes a denumeral collection of those members and excludes a denumeral collection of them, then there will be no two among all such pure abnumerable collections of which one follows next after the other or of which one next precedes the other, according to that rule. For example, among all decimal fractions whose decimal expressions contain each an infinite number of 1s and an infinite number of 0s, but no other figures, it is evident that there will be no two between which others of the same sort are not intermediate in value. What number for instance is next greater or next less than one which has a 1 in every place whose ordinal number is prime and a zero in every place whose ordinal number is composite? ·1110101000101000101000100001 etc. Evidently, there is none; and this being the case, it is evident that all members of a pure second-abnumerable collection, which both contains and excludes among its members first-abnumerable collections formed of the members of a pure first-abnumerable collection, cannot, in any *such* way, be in any linear series. Should further investigation prove that a second-abnumeral multitude can in *no way* be linearly arranged, my former opinion that the common conception of a line implies that there is room upon it for any multitude of points whatsoever will need modification.

Certainly, I am obliged to confess that the ideas of common sense are not sufficiently distinct to render such an implication concerning the continuity of a line evident. But even should it be proved that no collection of higher multitude than the first abnumerable can be linearly arranged, this would be very far from establishing the idea of certain mathematico-logicians that a line consists of points. The question is not a physical one: it is simply whether there can be a consistent conception of a more perfect continuity than the so-called "continuity" of the theory of functions (and of the differential calculus) which makes the continuum a first-abnumerable system of points. It will still remain true, after the supposed demonstration, that no collection of points, each distinct from every other, can make up a line, no matter what relation may subsist between them; and therefore whatever multitude of points be placed upon a line, they leave room for the same multitude that there was room for on the line before placing any points upon it. This would generally be the case if there were room only for the denumeral multitude of points upon the line. As long as there is certainly room for the first denumerable multitude, no denumeral collection can be so placed as to diminish the room, even if, as my opponents seem to think, the line is composed of actual determinate points. But in my view the unoccupied points of a line are mere possibilities of points, and as such are not subject to the law of contradiction, for what merely *can be* may also *not be*. And therefore there is no cutting down of the possibility *merely* by some possibility having been actualized. A man who can see does not become deprived of the power merely by the fact that he has seen.

The argument which seems to me to prove, not only that there is such a conception of continuity as I contend for, but that it is realized in the universe, is that if it were not so, nobody could have any memory. If time, as many have thought, consists of discrete instants, all but the feeling of the present instant would be utterly non-existent. But I have argued this elsewhere. The idea of some psychologists of meeting the difficulties by means of the indefinite phenomenon of the span of consciousness betrays a complete misapprehension of the nature of those difficulties.

Added, 1908, May 26. In going over the proofs of this paper, written nearly a year ago, I can announce that I have, in the interval, taken a considerable stride toward the solution of the question of continuity, having at length clearly and minutely analyzed my own conception of a *perfect continuum* as well as that of an *imperfect continuum*, that is, a continuum having *topical singularities*, or places of lower dimensionality where it is interrupted or divides. These labors are worth recording in a separate paper, if I ever get leisure to write it. Meantime, I will jot down, as well as I briefly can, one or two points. If in an otherwise unoccupied continuum a figure of lower dimensionality be constructed,—such as an oval line on a spheroidal or anchor-ring surface,—either that figure is a part of the continuum or it is not. If it is, it is a topical singularity, and according to my concept of continuity, is a breach of continuity. If it is not, it constitutes no objection to my view that all the parts of a perfect continuum have the same dimensionality as the whole. (Strictly, all the *material*, or *actual*, parts, but I cannot now take the space that minute accuracy would require, which would be many pages.) That being the case, my notion of the essential character of a perfect continuum is the absolute generality with which two rules hold good, 1st, that every part has parts; and 2d, that every sufficiently small part has the same mode of immediate connection with others as every other has. This manifestly vague statement will more clearly convey my idea (though less distinctly,) than the elaborate full explication of it could. In endeavoring to explicate "immediate connection," I seem driven to introduce the idea of time. Now if my definition of continuity involves the notion of immediate connection, and my definition of immediate connection involves the notion of time; and the notion of time involves that of continuity, I am falling into a *circulus in definiendo*. But on analyzing carefully the idea of Time, I find that to say it is continuous is just like saying that the atomic weight of oxygen is 16, meaning that that shall be the standard for all other atomic

weights. The one asserts no more of Time than the other asserts concerning the atomic weight of oxygen;—that is, just nothing at all. If we are to suppose the idea of Time is wholly an affair of immediate consciousness, like the idea of royal purple, it cannot be analyzed and the whole inquiry comes to an end. If it can be analyzed, the way to go about the business is to trace out in imagination a course of observation and reflection that might cause the idea (or so much of it as is not mere feeling) to arise in a mind from which it was at first absent. It might arise in such a mind as a hypothesis to account for the seeming violations of the principle of contradiction in all alternating phenomena, the beats of the pulse, breathing, day and night. For though the *idea* would be absent from such a mind, that is not to suppose him blind to the *facts*. His hypothesis would be that we are, somehow, in a situation like that of sailing along a coast in the cabin of a steamboat in a dark night illumined by frequent flashes of lightning, and looking out of the windows. As long as we think the things we see are the same, they seem self-contradictory. But suppose them to be mere aspects, that is, relations to ourselves, and the phenomena are explained by supposing our standpoint to be different in the different flashes. Following out this idea, we soon see that it means nothing at all to say that time is unbroken. For if we all fall into a sleeping-beauty sleep, and *time itself stops during the interruption,* the instant of going to sleep is absolutely unseparated from the instant of waking; and the interruption is merely in our way of thinking, not in time itself. There are many other curious points in my new analysis. Thus, I show that my true continuum might have room only for a denumeral multitude of points, or it might have room for just any abnumeral multitude of which the units are in themselves capable of being put in a linear relationship, or there might be room for all multitudes, supposing no multitude is contrary to a linear arrangement.